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SELECT
DISSERTATIONS

ON SEVERAL SUBJECTS

OF

MEDICAL SCIENCE.

BY

SIR GILBERT BLANE, BART.

FELLOW OF THE ROYAL SOCIETIES OF LOND. EDINB. AND GÖTTIN.

OF THE IMPERIAL ACAD. OF SCIENCES OF ST. PETERSBURGH ;

AND OF THE ROYAL ACAD. OF SCIENCES OF PARIS ;

PHYSICIAN TO THE FLEET IN THE WEST INDIES AND NORTH

AMERICA IN THE LATTER PART OF THE AMERICAN WAR ;

ONE OF THE COMMISSIONERS OF SICK AND WOUNDED

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DISSERTATION VII.*

- I. *On the Effect of large Doses of the Carbonates of Potash in Gravel, with Remarks on their administration, particularly on the advantage of combining them with Opium, and other Narcotics ; also, on the virtue of Opium in the case of Diabetes and Intermittent Fever, and as an Alexipharmick.*
- II. *On the Use of pure Alkalies and Lime Water, in Disorders of the Bladder, Stomach, and Skin.*

WHEN alkaline substances were first recommended as remedies for stone and gravel, it was thought that they were efficacious only in their pure state. This proved a bar to the free use of them, for their causticity in that state, necessarily limited the dose. But having heard my respected colleague at St. Thomas's Hospital, the late Dr. George Fordyce, remark, that in his opinion the mild alkali was equally efficacious, and having heard the late ingenious Dr. Ingenhousz mention the great benefit he had derived in his own case from what was then called the *aqua mephytica*

* The substance of this article was published in the year 1811, in the Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, vol. ii. and iii.

alkalina, the name by which the solution of potash surcharged with carbonic acid was then known; having also met with an excellent Tract on the same subject, by Dr. Falconer of Bath, I determined to make trial of it, but without abandoning the occasional use of the pure alkali.

But this alkali, in the ordinary state of its impregnation with the acid, that is, in the proportion of seventy to eighty, and called the sub-carbonate, in which form it was at that time most commonly used, though not so exceptionable as in its pure form, is still so acrid and nauseous as greatly to restrict the dose. It occurred to me that this might be obviated by neutralizing part of the alkali, and thereby extricating a portion of the carbonic acid which has so remarkable an effect in reconciling ungrateful medicines to the palate and stomach. The usual method I followed for adults was to dissolve two scruples, or even a drachm, in two or three ounces of water, to sweeten this solution with two drachms of honey, and to direct it to be taken, with half an ounce of lemon juice, or vinegar, three times a day. I preferred honey to sugar, on account of the mucilage it contains, and because I heard the late Sir John Pringle say, that he believed it to be a good remedy in nephritic disorders. I have also found this quantity of honey, in many cases, sufficiently laxative to supersede the use of medicine for that purpose, and which the opium,

I was in the habit of adding to each dose, might render necessary. The alkali, with a higher impregnation of carbonic acid, that is, in the proportion of seventy to sixty, and called the carbonate, or bicarbonate, being less acrid, and less nauseous, did not so much require the addition of the citric or acetous acid, but containing not much more than one-half of the alkaline ingredient, a larger dose was necessary. The preparation of it is effected by adding a solution of carbonate of ammonia to the sub-carbonate of potash, and the former, in consequence of its weaker affinity to the carbonic, parts with it to the latter. When it is fully charged and reduced to the form of a dry salt, it is no longer deliquescent, a circumstance which renders its administration more commodious.

It may here be objected, that the virtue of a large proportion of the alkali is destroyed by this method of administering it. Of this objection I was well aware, but was led to disregard it from the following considerations. First, about this time, a person under my care, taking this alkali neutralized with lemon juice, in the form of the common saline draught, was, from curiosity and amusement, in the habit of trying the urine by the test of paper stained with litmus, and found that, under the use of this medicine, the urine came to lose its power of reddening the paper, in the same manner as happens under a course

of alkali. It would appear, therefore, that in the progress of the circulation, by virtue of some of the chemical changes arising in the processes of secretion and assimilation, this compound is decomposed, so as to allow the alkali to operate as if it had been swallowed separately. Secondly, Admitting that one third or even one half of the alkali were to be deprived of its medicinal quality, the portion which remains unneutralized is far greater than the palate or stomach could bear, in a separate state. Lastly, I found from experience, that the actual effects were as great, as could have been expected from the whole alkali, without the addition of an acid. It may further be stated, as a recommendation of the practice of administering the alkali in combination with the carbonic and citric acids, that morbid affections incident to a long use of the alkali alone are thereby prevented.

It was found advisable to adopt certain modifications and additions to this medicine, according to the variety of the accompanying symptoms. It happens not unfrequently, for instance, on the first attack, that the sharp angular concretions produce symptoms of acute nephritis, in which case the usual quantity of uncombined alkali should either be less, or omitted, till the inflammation shall have been subdued by bleeding, the addition of nitre, and other parts of the antiphlogistic treatment.

But the most important peculiarity in my method of administering the medicine consisted in the addition of the opium.

When there was great pain and irritation, without the suspicion of inflammation, I found this not only admissible, but highly advisable ; inasmuch, as it not only alleviated the pain, but if given for a considerable time, contributed materially to expedite and complete the cure. It is, indeed, this fact which has chiefly induced me to lay this Dissertation before the public. I could not help being much struck with it, but was the less surprised on reflecting, how analogous it is to what I had observed, with respect to the effects of opium in other cases of irritation. I will also confess, that I was further induced to adopt this practice, from finding that a medicine sold as a secret, evidently consisting of alkali conjoined with opium, had in some cases of gravel been more effectual, than the alkali directed by myself without this addition. I have, therefore, for several years been in the habit of adding from seven to fifteen drops of *vinum opii* to each of the doses of the alkali, and am fully satisfied, that it not only prevents the distress arising from irritation, and facilitates the discharge of calculi, by relaxing the spasms of the ureters, but that it renders the cure more expeditious, more certain, and more permanent. In those constitutions which do not bear opium, hemlock has been found

a useful substitute. I recollect hearing Dr. Black, in his Lectures on Chemistry, which I attended in the year 1770, in the University of Edinburgh, mention that hemlock was a remedy in gravel, but whether in his own experience or not, my memory does not serve me. Dr. Prout, in an ingenious and elaborate work on urinary concretions published last year (1821), says that hyosciamus is eminently useful, particularly in those cases of concretions in which lithate of ammonia prevails.

I have elsewhere remarked,* that, in ill-conditioned ulcers, in the West Indies, opium was found superior to all other internal medicines for producing a disposition to heal. Under the free use of it, such ulcers would in place of a sanious discharge produce a healthy pus, succeeded by granulations and cicatrization. Opium appears to do this by suspending irritation, and perhaps by promoting absorption. Nor is this fact more wonderful and unaccountable, than the other changes produced in animal fluids, by the contact and action of the living solid parts in the functions of assimilation and secretion,† whether in a state of health or disease, to which there is nothing analogous in the habitudes of dead matter, unless the late discoveries of the chemical powers of

* Observations on the Diseases of Seamen, p. 520, 3d edition.

† This subject has since been successfully illustrated in some well instituted experiments made by Mr. Brodie, in the Croonian Lecture of 1810, and printed in the Phil. Trans. of 1811.

electricity can be reckoned such. I afterwards, on these grounds, and in consequence of a case* related to me by Dr. Nooth, made trial of a free

* This case is as follows. About thirty years ago, a young medical man was affected with a phagedenic bubo, and went into the country under the conviction that it would prove fatal, as he had observed, in the course of his education, that this was the usual termination of similar cases. In order to allay his sufferings of mind and body, he determined to make a free use of opium, and raised the dose far above what in common practice was considered as advisable, or even safe, setting no limits to it but the attainment of ease. The consequence was that the ulcer began to suppurate kindly, and finally healed. Dr. Nooth, to whom this case was known, went soon after to America, as Physician to the Army there, and took pains to communicate the knowledge of this to the medical officers of the army. A physician, belonging to the Hessian troops, who then served in America, conceived from misapprehension, that opium was a remedy for the venereal disease in all its forms, and very hastily, and most reprehensibly, published a small tract to this effect. The novelty of the practice, and confident tone of his work, procured it a wide circulation and some credit, insomuch, that, when I returned to England after the war, I found that some of the most eminent practitioners had been making trial of it in virulent gonorrhœa, and chancres, either in their private or hospital practice. The fallacy was soon detected; but the remedy shared the fate of many other excellent medicines. Not having been found good *for every thing*, it was condemned as good *for nothing*, and discarded, so that the practice was neglected, or but little attended to, in those cases to which it is really adapted, for which it had been originally intended, and in which it will ever be found of benefit.

The employment of opium, in venereal ulcers, is very accurately stated in a pamphlet published by Mr. Grant, entitled ‘Observations on the use of Opium, in removing Symptoms supposed to be owing to Morbid Irritability.’ London, 1785.

use of opium in phagedenic buboes, while I was Physician to St. Thomas's Hospital. This was attended with great success; and I find in my notes one case in particular, which after being in another Hospital for six months, and dismissed as incurable, was cured at St. Thomas's by this practice. The largest quantity the patient took daily was two grains in the morning, as many in the middle of the day, and five at bed time; but I have seen it carried further. Mercury was found to aggravate the symptoms; and I believe that the chief cause of these untoward cases is the excessive use of it, and probably the less frequent occurrence of them of late years is owing to the more moderate and judicious use of mercury.

I need not remark to those, who are in the habit of reflecting on the principles of the animal œconomy, and the operation of medicines, that all the healing processes are ultimately and essentially the work of nature, that the means employed by art consist merely in enabling nature to perform these processes, or in removing such obstacles as impede her operations, and that of these obstacles one of the chief is irritation. Upon this principle, it can as readily be conceived, how the morbid action generating gravel may be increased by the irritation of that gravel, as that a sanious discharge should be kept up and increased by its own acrimony. It may also be remarked, that the urinary organs are more liable to nervous influence, and consent of parts, than most others,

as is exemplified in the sympathy of the stomach from calculous irritation, and the great quantity of pale urine secreted in hysterical and other nervous affections. It is not so evident, however, in point of reasoning, how the cure of gravel by this practice should be more permanent. It might indeed be alleged, that a change produced by a medicine affecting the action of those solid parts, which are the seat of sensibility and irritability, is less likely to be transient, and more likely to be permanent, than one which merely produces temporary chemical changes on the fluids. But as the fact in question was ascertained by experience, without having been suggested by plausible speculations, I wish to rest it on the former. That this is the only legitimate criterion of practical reasoning, and that chemical experiments out of the body are very fallacious, may be illustrated by another fact belonging to this subject. A gentleman, subject to frequent fits of gravel, was in the habit of making experiments on the small concretions which he passed. He found that soda dissolved these, but that potash did not; nevertheless, he experienced sensible relief, and even temporary cure, from the internal use of the latter, but no benefit from the former.* As far as I can judge of the comparative powers of

* A remarkable instance of the inefficacy of soda, though given in large quantity, is related in Mr. Home's Observations on Mr. Brande's Paper on the Structure of Calculi, inserted in the Philosophical Transactions for 1808.

the two alkalis, I should greatly prefer the potash to the soda, as a remedy for the cases in question. One reason of this may perhaps be, that soda is an element of the animal fluids, as it enters largely into the composition of the bile, so that it is more likely to be arrested in the course of circulation, and diverted from the urinary organs. I may observe by the way, that the patient above-mentioned has for a series of years been subject to frequent relapses, but that I never could prevail on him to use opium, as he has, like many others an insurmountable objection to this drug.

I hope what I have said in favour of opium will not be considered, as giving countenance to that indiscriminate use of it in various internal complaints, which has of late prevailed in medical practice, in consequence of some hypothetical doctrines that have been propagated. Let it not be forgotten, that the maxim, that *the best things are the most liable to abuse*, is peculiarly applicable to medicines. Its beneficial effects are limited to cases, in which there is spasm or irritation in some form or other, either manifest or obscure. I have found it uniformly hurtful, where there is either inflammation or simple debility, that is, debility proceeding from a natural failure of the powers of life, and it probably never proves cordial or exhilarating, but where there is some spasm visible or latent. It has a striking effect in those cases or constitutions, where chilliness,

and languid circulation, are the predominant symptoms ; and what is chilliness, and the sensation of cold, but a spasm of the extreme orifices of the cutaneous vessels, as is manifest from that rough constriction of the integuments vulgarly called *goose-skin* ? The reader is referred for a still further proof and illustration of this to what has been said of opium in the cure of intermittent fevers at page 242 of volume i.

It is not foreign to the present subject to observe, that opium has a remarkable power in mitigating the effects of poisons. Dr. Reynolds* found, that the danger arising from the internal use of the cerussa acetata is obviated, by conjoining it with a small quantity of opium. I have experienced the like good effect from this combination. I have employed it with the same intention, and with the like good effect, in obviating the virulent powers of arsenic ; and it is fortunate, with respect to intermittent fever, that the virtues of both these concur, having been found separately, as well as jointly, powerful remedies in that disease. In illustration of this, I beg to give the outlines of a case, which occurred to me a few years ago. In the year 1800, there was a camp at Swinley, near Windsor, in which an intermittent fever appeared, and it was observed that those only were affected with it, who had served in the

* Medical Transactions of the College of Physicians, vol. iii. page 99.

campaign of North Holland, in the autumn of the preceding year, affording a curious and well ascertained exemplification of the reality of latent predispositions. One of the officers, affected with a tertian intermittent, came to town to place himself under my care. Having a pain in his right side, which was suspected to have arisen from a chronic affection of the liver contracted in a tropical climate, where he had served a few years before, I was desirous of curing him without the bark. After the usual evacuations, therefore, he took opium before the fit, in the manner already described, and also every night at bedtime, by which the paroxysms were mitigated, but not prevented, at the end of the first week. I then directed ten drops of Fowler's solution* of arsenic to be given every four hours, after which he never had a rigor, but only an uneasy jactitation at the usual time of the paroxysms; and this disappeared upon raising the dose gradually to fifteen drops. This dose was continued for a few days, and the disease was entirely removed, without the least inconvenience having been felt from the remedies. As the dose of arsenic here administered was considerably above what is regarded as advisable, or even safe, I have no hesitation in ascribing this safety to the opium, par-

* A preparation of this mineral of the same strength has been inserted in the edition of the London Pharmacopœia of 1808.

ticularly as I have observed the same effect in other cases.

It seems worth enquiring, whether the deleterious effects of vegetable and animal, as well as the mineral, poisons, might not be prevented by opium. We know that the ancient compositions called alexipharmaca and theriaca, in which opium was the most important ingredient, derived their appellations from their real or supposed powers of counteracting poisons ; and it would be difficult to say, upon what grounds the moderns have pronounced so hardily, that the confidence of the ancients in these compositions was founded in credulity and superstition. If I had the misfortune to be bit by a mad dog, I should place much more reliance on due doses of some of the officinal opiates, taken habitually, till the predisposition might reasonably be supposed to be obliterated, than on any thing that the moderns have suggested as an antidote to this poison, being seriously of opinion, that opium affords a very likely means of counteraction, particularly if combined with arsenic, which has been proposed as a remedy in this disorder by Dr. Hunter.* We have seen how well these two medicines accord in the cure of agues ; and it may be remarked, that the remedies for ague partake, in their

* See Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, vol. i. p. 294. London, 1793.

application and operation, of the nature of prophylactic means ; for they are administered, during the absence of the paroxysms, with a view to prevention, in a complaint of which the remote cause is a morbid poison.

A few other practical remarks may be made, before dismissing the consideration of this interesting article of the *materia medica*. One is, that I have not seen it manifest any of its peculiar properties, whether local or general, that is, any narcotic, anodyne, anti-spasmodic, or exhilarating effects, except when brought into contact with some portion of the alimentary canal.* The next is, that in those constitutions, in which it ordinarily produces nausea, head-ach, febrile heat, and other distressing sensations, it has not in general these effects, when such constitutions labour under severe spasm and irritation, which it is then equally effectual in removing, as in any other constitution. Lastly, the effect of opium in diabetes, is highly illustrative of the principle, which it has been one of the chief objects of this communication to establish, namely, that the removal of irritation produces not merely a palliative and temporary, but a radical and permanent effect. Only one case of this disease has occurred to me, since I became more fully acquainted with the powers of opium. It was in a female, who had

* See an important practical observation on this subject in *Trans. of Med. Chir. Society*, vol. v. p. 154. London, 1814.

laboured under the complaint for fourteen months, and, under the use of ten drops of vinous tincture of opium thrice a day, recovered entirely in three weeks, and she has remained well now a year. Some other remedies usually directed in this complaint were taken at the same time, but their want of success, in so many other cases, leaves little or no doubt, that the striking benefit, derived from the treatment in this case, was owing to the opium. The diet, purely animal, which I enjoined, had probably the next greatest share in the cure. I also directed oily friction all over the skin, having seen evident benefit from it in like cases.*

In the title of this paper, I have made use of the term *gravel*, meaning to distinguish the disease so called, from the stone. The recent concretions known by the name of gravel are generally accompanied at their first formation with nephritic pains, and have been found to consist of uric, otherwise called lithic acid, except in a very few cases, in which they are composed of † oxalate of lime; whereas most of the large concretions, which have been forming for a length of time in the bladder, are found to consist, besides, of a

* See an article in the 4th volume of the Transactions of the College of Physicians on the use of opium in Diabetes, by Dr. Pelham Warren.

† See Mr. Brande's Letters to Mr. Home on the Structure of Calculi, in the Philosophical Transactions of 1808.

variety of other ingredients; and some of them contain no oxalate of lime. The discoveries on this subject do great honour to the science of this age, and are of great utility in guiding our opinions and practice, in the treatment of the complaints under consideration. The concretions of uric acid most frequently form the nucleus of stones; and it has been computed, that a fourth part of stones, found in the bladder, consist of uric acid, though Mr. Brande thinks this computation too high.* The great majority of stones are found, besides this acid, to contain ammonia, magnesia, silica, lime, the phosphoric and oxalic acids, in various combinations and proportions. A few have been found without any uric acid. In consequence of these laborious and successful researches, we are enabled to understand, why alkaline bodies are not in all cases equally effectual in curing or relieving calculous complaints. This had so far disparaged their character, as to produce a general diffidence of their efficacy in any case whatever. It is, therefore, highly important to mankind, that the extent of this efficacy should be ascertained, in order that practitioners and patients may neither be too sanguine, nor too despondent, with regard to the benefit to be expected from them. And I must here again remark, that, whatever deference may be due to the lights thrown upon this subject by

* See Journal of the Royal Institution, vol. vi.

chemical science, the effects of remedies on the living body are not to be decided exclusively on this principle. Notwithstanding its being found, that the greater part of the substances, of which stones consist, are incapable of being acted upon by the fixed alkalis, it is conceivable that alkaline remedies may be useful in all cases, if we are to admit what has been plausibly alleged by some theorists that, they act rather by correcting the powers of digestion, than by their chemical property of neutralising morbid acidity. The opinion of there being any remedy that can properly be called a solvent, is now laid aside by all correct reasoners and practitioners. But though there may be no proof of their having dissolved a stone, properly so called, yet they frequently do what is equivalent ; for by preventing fresh concretions, which from their crystalline form are extremely sharp, the stone becomes so smooth, as to lead to an opinion that it has actually been dissolved. This was particularly remarkable in the case of Lord Walpole, treated and described by Dr. Whytt about the time when this medicine was first introduced into regular practice. This patient was relieved from the sufferings of the stone for several years before he died, which was at the age of eighty, although the stone was found in the bladder after death.*

* See Dr. Whytt's Works ; also two remarkable cases of
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It is, certain however, that the neutralising property of alkaline substances does reach the urinary secretion; for, by the internal use of them the urine loses its quality of reddening the blue tests. Nor, can it be doubted that alkalies, magnesia and lime have the effect of aggravating cases in which the concretions consist of the triple phosphates, and ought here to be avoided. The other positive improvements in practice suggested by the intimate knowledge of the ingredients of these concretions have been the successful use of magnesia in cases of the uric acid, and the use of acids, vegetable or mineral, in those cases in which the prevailing elements are alkaline, generally distinguished by the excretion of white sand as the others are by that of red sand. A scruple of carbonate of magnesia, twice a day, has been found to answer as well as the alkalies; and nitric, or muriatic acid, from five to fifteen minims thrice a day, largely diluted, have proved eminently useful in the appropriated cases. I shall conclude with remarking, that all the substances found in the composition of stone, except the uric acid, of which there is a certain proportion in the most healthy urine, seem to be the creatures of morbid irritation.* In almost every

cures by lime water and soap, by the same author, in the *Medical Essays of Edinburgh*, vol. vi. page 159 and 278.

* To assert this kind of creative power of secretion seems a bold position. It can, nevertheless, be maintained with consi-

instance, the concretions of uric acid form the nucleus of stones, which would not be the case, if the other substances were produced independently of irritation from the stone itself. The specific effect of this irritation, as the stone advances in size, is to produce, by exciting morbid secretion, the various other compounds which have been enumerated :* and there seems to be

derable plausibility, and even probability, if not with certainty. The natural history of calcareous earth lends great support to this hypothesis ; for, it is not conceivable that the vast masses of shell which compose whole mountains of chalk and other masses of carbonate of lime so immense as to constitute a sensible part of the globe of the earth, could have been derived from such a quantity of it as could enter into the food of the animals of which they are the *exuviae*. And if the organs of living creatures can create such enormous mounds, what is to hinder them to create such minute portions of lime, silica, and magnesia as enter into the morbid concretions in question? Mr. Hatchett, so justly celebrated for his experimental precision in chemical analysis, in conversation with me, adduced the following fact in support of the same hypothesis. A chick, just extricated from the shell, possesses bones which consist of phosphate of lime, though these elements could have no access to it during its growth. And does not a familiar and undeniable fact in the history of land animals lend some countenance to this opinion, however incredible at first sight? for, is any thing better known than that the soft parts of animals, quadrupeds for instance, which are nourished by substances so different as animal and vegetable matter, do nevertheless consist of matter of the same chemical composition?

* I am glad to find my ideas respecting the operation of opium, and the action of irritation in generating and modifying

particular stages of its growth, at which it produces one compound rather than another, as appears by the strata of urinary stones. The several strata may be considered as expressions of this fluctuating action, so as to be registers, as it were, of the duration and succession of these actions, in producing the several species of matter composing stratified stones. The stratum of oxalate of lime, which gives a mulberry appearance to the stone, is seldom formed till the stone has attained a considerable size, being rarely found in the central parts of these concretions.* This being the case, whatever tends to obviate irritation, will likewise tend to prevent the generation of calculous matter of every description except uric acid. Both the medicines I have mentioned possess the property of diminishing irritation; for not only has opium this effect, but also the alkali, as I shall endeavour to prove in the next part of this Dissertation; and the aqua mephitica alkalina has been found to remove† bloody urine

urinary concretions confirmed by so respectable an authority as that of Dr. Prout, in his Enquiry into the Nature and Treatment of Gravel. Lond. 1821.

* It makes no difference, either in the reasoning here employed, or in the practice proposed, whether the constituents of stone are derived from the urine, as secreted in the kidneys, or, as is alleged by Dr. Austin, from the surface of the bladder. See a Treatise on the Origin and Component Parts of the Stone in the Urine of the Bladder. London, 1791.

† See Dr. Falconer's Cases before quoted.

and strangury, though not proceeding from gravel or stone.

II. It was remarked in the first part of this Dissertation that the alkaline remedies, in their mild forms, that is, combined with carbonic acid, had in some measure superseded their use in their pure state. But I have from experience seen good reason why the latter ought not to be entirely abandoned; for I have found it in several instances more efficacious than the other. In looking over my notes, I find a case which occurred to me ten years ago, where the carbonated potash had been fairly tried without any good effect; but the complaint readily yielded to the alkali in its pure state. The main objections to it are its acrimony and nauseousness. These should be covered by mucilaginous and sweet substances, some mild aromatics, and large dilution.* Another alleged objection to it is, that its long continued use brings on a disordered state of bowels of a peculiar nature. Magnesia, though the mildest of all such remedies, is not without a

* The following may be taken as a specimen of my formulas; Recīpe aquæ puræ vel cujusvis aquæ distillatæ aromaticæ, mucilaginis gumm. acaciæ āā f. ʒij. Mellis despumatæ ʒiss. liquoris potassæ f. ʒiii. Vini opii m. xl ad ʒi. M. Sumantur cochlearia duo magna e poculo decocti hordei ter in die. Interdum addantur singulis dosibus spiritus juniperi compositi f. ʒij vel potassæ nitratae ʒi.

similar objection ; for, the protracted use of it has been known to produce hard concretions causing considerable disturbance in the intestines. I have known one case of this, and more are mentioned in the Journal of the Royal Institution.* I have sometimes advantageously combined the pure alkalis in half the above stated dose with two or three ounces of lime water, blunting the acrimony with a spoonful or two of milk. By the formula, it will be seen that it has been my practice to combine opium with the pure, as well as with the carbonated alkali. And I beg to repeat, that secretion, whether healthy or morbid, being an action of life, is more likely, considered *a priori*, to be affected by causes acting on sensibility and irritability than by agents merely chemical, and that this is confirmed by experience.

In the year 1796, I was induced to lay before a private society† which met monthly for conversation and the collection of papers on subjects of medicine and surgery for publication, some ob-

* Vol. i. page 297.

† This is the same Society alluded to in the first part of this Dissertation. It consisted of the justly celebrated John Hunter, its founder, Dr. George Fordyce, Dr. John Hunter, Dr. David Pitcairn, Dr. Baillie, Dr. Andrew Marshal, Sir Everard Home, Dr. Patrick Russell, and myself. Three volumes were published by this Society under the title of Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, between the years 1793 and 1812. The founder and most of the above mentioned members contributed to the collection.

servations on the effects of pure alkalies and lime-water in several disorders, not including stone and gravel complaints, in which their virtues were chiefly known. The cases alluded to were those of urinary irritation not depending on calculous concretion, indigestion, and certain cutaneous affections. My attention was drawn to the first of these by the following incident :

A captain of a line of battle ship, in returning to England from the West Indies, after the peace of 1783, was seized with a fever, for which, among other remedies, a blister was applied. The usual means of preventing strangury having been neglected, he was much distressed by that symptom, and he continued for two years afterwards to be greatly troubled with a frequent desire to make water, which he passed with some degree of difficulty and uneasiness. He tried a variety of remedies to no purpose, so that being in a great measure prevented from going into company, and his life being extremely uncomfortable, he retired into a remote part of the country. There he consulted a practitioner who advised him to use the *aqua kali puri*, taking his complaint most probably for the gravel. Immediately upon the use of this he began to recover, and very soon got free of his complaint.

After his return to town he related this to me, and profiting by the hint, I have several times since employed the same remedy with like success

in cases of a similar kind. The cases I mean are those in which there is no suspicion of stone or gravel (the only cases in which these medicines have commonly been thought useful) but where the complaint proceeded from a preternatural tenderness or irritability of the urinary passages and bladder.

I at first considered this as an empirical matter of fact, or perhaps a mere casual coincidence, no mode of accounting for it having then occurred to me. The late experiments in chemistry, however, seem to throw light on the *rationale* of this practice; for it is found that urine, in its most healthy and natural state, contains a certain portion of that peculiar acid,* which, when superabundant, concretes into gravel or stone. Even when the body is in a morbid state, it is not often found that the urine puts on an alkaline character.

* This acid was first observed and proved to exist in the urine, by Scheele in Sweden, about the year 1776; and its nature has been further illustrated since by Cadet, in France, and Mr. Murray Forbes in England. But the greatest stride made by any chemical philosopher in the analysis and classification of the several urinary concretions was by Dr. Wollaston. The account of his discoveries is published in the Philosophical Transactions for 1797, and led the way to further researches which have placed this branch of animal chemistry on the most clear and satisfactory foundation, from being one of the most dark and imperfect. The first complete exposition of this subject was in Dr. Marcet's Treatise on Urinary Concretions. Lond. 1817.

It seems probable, therefore, that the irritating property of the urine in this disease depends upon the acid, and that the alkali acts by neutralizing it. That the acid of the urine is, by the use of the alkali, either neutralized or prevented from being formed, is a matter of demonstration, for under the use of it the urine loses its power of reddening the blue vegetable tests, as already remarked.

May not the superior efficacy of the pure alkali over other absorbent substances depend upon a chemical fact discovered by Scheele, that the acid of the urine has a weaker affinity with alkalies, than even the carbonic acid; and may not the same be true of the gastric acid? For an alkali or an earth already combined with an acid, cannot act as an absorbent on any other acid with which it has a weaker affinity. The property of the urine by which it stimulates the surface of the urinary passages, cannot be reckoned morbid, any more than the acidity upon which it depends; for a certain degree of specific stimulus is necessary to every species of animal fluid, in order to excite the contractions of their containing cavities and vessels, either for the purpose of expulsion or propulsion, in which their respective functions consist. There are other contents in the urine, such as common salt, the phosphoric and ammoniacal salts, which have more sensible acrimony than this acid; but it does not follow that they

have more effect in stimulating the bladder, for the peculiar power of fluids in stimulating their containing vessels and cavities does not depend on their simple acrimony. The natural state of the urine therefore being given, the morbid effect will depend on the preternatural sensibility of the cavity or passage with which the urine comes in contact.

These facts, and the explanation given of them, throw light also on a circumstance already adverted to, and known to most practitioners, that the alkaline medicines given as lithontriptics mitigate and even remove the symptoms of the complaint, though the stone remains undiminished in the bladder. This arises, I apprehend, not only from the smoothness of the stone, in consequence of no new accretion being formed, but from the effect of the medicines in diminishing the acrimony of the urine.

What has been said of the pure alkali will also apply to lime-water; for though I have not equal experience of this last, yet I have seen manifest advantage from it, and have sometimes employed it interchangeably, sometimes in conjunction with the alkalies.

One of the most remarkable instances of relief from these medicines was in the case of a patient, who, in consequence of strictures of the urethra, came to be affected with an opening in that passage *in perinæo*, and had at times laboured under

dangerous suppressions of urine. He was also of what is called a scorbutic, that is impetiginous habit, and had for many years had symptoms of gravel. In this state I directed a course of lime-water and caustic alkali, as the pure alkali was then generally called, under which he has for several months been free from suppression of urine, the whole of it passes the natural way, in so much that the lateral opening seems to be healed, and the strictures having been in some measure removed by bougies, which he had used more or less since the beginning of this complaint, his life has been tolerably comfortable.

The good effects of the medicines in this case were most probably owing to the destruction of acid acrimony, of which there were strong indications in the habit of this patient, for he was affected with a most intense acidity of stomach, which was cured by these remedies, and a course of diet, consisting exclusively of animal food, sea biscuit, and rice, with brandy and water for drink, a treatment from which I have found the utmost benefit in similar cases, but generally with the soda instead of the potash.

In such cases it is most prudent to begin with small doses, for in full doses I have known this medicine brought into discredit, and laid aside at the first trial, in consequence of the irritation it caused by increasing the quantity of urine, an effect which it sometimes has. The dose is from

fifteen to thirty drops, thrice a day. The benefit arising from these medicines usually appears in a few days, but they must be continued from six weeks to three months to have a permanent effect. The substitution of the carbonated alkali for mitigating these irritations, is as advisable as in the case of gravel. The experience of Dr. Falconer on this point has been already mentioned at page 20.

2. The next complaints I have to mention, in which I have observed these remedies to be useful, are certain cases of indigestion, especially those in which acidity is the prevailing symptom. In these I have had the most unequivocal proofs of the good effects of lime water and pure alkalies, the former being given in doses from three to four ounces, twice or thrice a day, and the latter from twenty to thirty drops, as often, diluted in a watery vehicle. I have observed a manifest superiority of the pure mineral alkali or natron, now called soda, over the pure vegetable alkali, or kali, now called potassa, in stomach complaints, and I should think the former deserves a place in our dispensatories. This course of medicine should be accompanied with an appropriate diet, as stated above.

In reasoning upon these facts, the most obvious idea that presents itself is, that these medicines act by destroying acidity. But on account of their acrimony, the quantity in which they can

be taken is so small, that this absorbing power, with regard to the general contents of the stomach, is too inconsiderable to account for the effect; and if it depended upon this alone, the mild alkalies, magnesia, and the common calcareous earth, ought to have much greater power, as they can be taken in much greater quantities. This however is not the case, except in some of the complaints of children depending on acidity, which are cured by large doses of the earthy absorbents. But in the chronic diseases of the stomach in adults, these have merely a palliative effect, while the pure alkalies and lime water have an evident effect in correcting the function itself of the stomach, and thereby producing permanent relief.

But though these medicines cannot be administered in sufficient quantity to have a sensible effect on the contents of the stomach, consisting of the *ingesta* rendered acid by vitiated digestion, yet it is conceivable, and even highly probable, that they may act on what may be called the *gastric acid*, which may be secreted in so concentrated a state, or in such quantity, as to constitute disease. I have satisfied myself that there is such an acid, by applying the usual tests to the inner surface of the stomach of animals. This property in ruminating animals is confined to the digesting stomach, and is found not only in the stomachs of those recently killed, but of those which

have been preserved in pickle, as in that of a calf which has been cured for rennet. There can be no doubt but the power which this possesses of coagulating milk is part of the digestive faculty inherent in it: and though this faculty in its full extent has been abundantly proved by Dr. Fordyce* to depend upon the living power, yet it is evident, from the stomach of the calf retaining the coagulating property so long, that this is an effect purely chemical, that is, consisting in the mutual action of dead matter upon dead matter. It would be losing time to pursue these speculations any further, for the practical conclusions do not at all depend upon them, but upon observation and experience.

The only other remark I know of, relating to this part of the subject, is, that lime water is sometimes eminently useful in chronic dysenteries of the lenteric kind, and it probably acts by correcting the functions of the stomach. This is no new remark of mine, but I can testify the truth of it from my own experience.

3. There is another class of disorders in which the good effects of lime water and pure alkalies have not hitherto, in so far as I know, been sufficiently attended to. These are certain cutaneous complaints, particularly those affecting the face, and commonly called *gutta rosea*. I do not know whether it is conformable to the observation of

* See Treatise on the Digestion of Food. London, 1791.

others, but I have found cutaneous complaints so frequently connected with calculous complaints, that I cannot help inferring that the same constitutions are liable to both. This is more particularly the case with those impetiginous affections which depend on an hereditary constitution, and incident to what is called a scorbutic habit.

Cutaneous complaints are still oftener connected with complaints of the stomach. This is most commonly observable in eruptions of the face, which very usually coexist or alternate with indigestion. It is also very common to observe more general defœdations of the skin connected with hypochondriasis and hæmorrhoids, both of which may generally be considered as impetiginous affections of the intestines. But in so far as I have observed, it is in those cases more particularly which affect the face, that the Lime Water and Alkalies are beneficial; and it seems probable, that these medicines act upon one and the same principle in the good effect they produce, both in the external and internal complaints.

I have also, however, seen some remarkable cures of herpetic complaints of the legs, by large doses of lime water, not less than three pints a day. I was induced to try it in such cases, by a very striking one related in the first volume of the London Medical Observations and Enquiries, published in the year 1757.

All the excretions of the skin which I have

tried by the usual tests have shewn an acid character. Whether this has any connection with the operation of the medicines I will not pretend to determine; but in some cases of eruptions I have employed the lime water as a wash with manifest good effects, so as to have effected a cure after mercurial and sulphureous applications had failed.

There are certain species of boils not included in the description of cutaneous disorders which I have here in view, but in which I have heard the late celebrated Mr. Hunter say, that he had observed the use of pure vegetable alkali to be followed with relief in such a number of cases, as to render its efficacy undeniable; and he first discovered this by observing boils to be cured in a case, in which this alkali was given for a calculous complaint.

The only other disease, so far as I know, in which the medicines in question have been supposed to be beneficial, is the gout, and next to calculous complaints their virtues in this, I believe, have been most generally allowed. But as it was my intention to point out only such effects as have been less commonly attended to, I shall not enlarge upon this, and shall only remark, that in some cases they seemed to protract the intervals of this disease, while in others my expectations were entirely disappointed. There is a very impressive account of the cure of gout by

the daily dose of a quart of lime water, in the third volume of the Physical and Literary Essays of Edinburgh. The strong tendency to acidity in the stomach in the gouty diathesis renders absorbents highly useful, though they may not effect a cure.

DISSERTATION VIII.

On Muscular Motion ; being the Substance of the Croonian Lecture. Read before the Royal Society, by the appointment of the President and Council, on the 13th and 20th November, 1788.

Importance of the subject—Extensive influence of Muscular power in the animal economy—Properties of a Muscle in its dead state—Considered in relation to other motions—Motion an original property of all matter—Proofs of the inherently active nature of all matter—Mechanical impulse cannot be a primary cause of motion—Hypothesis of Attraction and Repulsion constituting the essence of matter—Muscular motion cannot depend on any mechanical cause—But an original law of Nature—Proofs of its increasing the power of cohesion—Experiment—Whether Muscular contraction increases density—Experiments—Contraction produces no change of density—nor of temperature—Recapitulation—A Theory of Muscular contraction—Enumeration of *stimuli*—1. Internal *stimuli*—All the functions of the body carried on by specific *stimuli*—Application of this to the resuscitation of life—And to Pathology—Exemplified in the absorbents—Analogy between motion and sensation—Whether vitality is dependent on the Nervous System—The mutual dependance of simple life and nervous influence—Essential properties of simple life—Nervous influence unfriendly to simple life—This illustrated by observations on fish—on quadrupeds—and by diseases—Fatigue—Sleep—Effects of immoderate labour, and of sensuality—The practical benefit of removing irritation—*stimuli* connected with consciousness—Effects of the Passions on muscular fibres—2. External *stimuli*—Instinct—Habit—Correspondence between the properties and laws of external Nature and the faculties of animals—Imitation—Necessity of Tension—its excess and defect as producing disease—Muscular Motion considered mechanically—The strength and stature of the body adapted to external nature—Advantage and disadvantage of their insertions and obliquity—Advantage of obliquity demonstrated by a geometrical theorem.—Conclusion.

THIS Lecture was founded by Dr. Croone, who was one of the original Fellows of the Royal

Society, having been previously a member of those private meetings which laid the foundation of this institution. He was not only a physician of learning and eminence, but his character for taste, as well as for mathematical and natural knowledge, was so distinguished, that he was elected Professor of Rhetoric in Gresham College,* and was appointed a member of the first council of the Royal Society. What prompted him to perpetuate and keep alive an attention to this subject was, no doubt, an opinion of its importance and difficulty. There are certain branches of knowledge which, being considered as belonging to particular professions, appear less interesting to men who entertain a taste for general science; and this has in some measure been the case with inquiries relating to the animal œconomy. But when we consider the rank which animated beings hold in the scale of Nature, and that Muscular Motion involves some of the most important circumstances relating to them, it cannot be denied that this is a subject highly interesting, as a branch of natural knowledge in general, independent of its utility as subservient to medicine. For, though sensitive beings bear no assignable proportion to the great volume of the material world, yet as man belongs to this class of existence, and as all other existence would seem to be created in vain, unless there were beings capable

* See Ward's Lives of the Gresham Professors.

of perception and enjoyment, the investigation of animal nature appears to be of the utmost importance, not only as the grounds of a useful art, but as an object of a philosophical curiosity, as intense and interesting as any in the compass of universal Nature.

Muscular motion is justly deemed an important and a characteristic attribute of animated beings, not only as conferring that loco-motive faculty peculiar to animals, and that power by which they are enabled to exercise a command over external objects, but also as it constitutes that energy by which the motion of the fluids and all the internal functions of the body are carried on. For, we are to consider as muscular not only those large masses of flesh which compose so great a proportion of the whole bulk of the body, but likewise all the minuter organs subservient to circulation, nutrition, and secretion ; since not only the heart itself, but the whole vascular system and the intestines, owe their action to certain powers of irritability and contractility peculiar to living fibres, all referable to one class of organs.*

* Since this was first written, some physiologists have questioned the propriety of calling vascular and membranous organs muscular, though possessed of living contractility, on account of their chemical composition being different from that of the organs commonly denominated muscles. This seems to be a matter of mere arrangement and definition, and the sense in which I wish to employ the term, includes all

In investigating this subject, it seems most natural to begin by comparing the muscles, and the motion belonging to them, to other modifications of matter and motion that occur in nature.

A muscle, even in so far as its structure is an object of our senses in its dead state, has characters which distinguish it from every other substance in nature. The most striking of these is its regular organization of parallel fibres. The fibrous structure is, indeed, found in other parts of the body, such as the tendons and ligaments, and also in vegetables, some of which are even possessed of visible irritability; and a similar conformation is manifest in some minerals, such as the *asbestos*; but there is a certain degree of tenacity, elasticity, and moisture, which, joined to its fibrous organization, distinguish it from every other form of matter. With regard to the minute structure of muscles, though some have fancied they have seen, by the help of glasses, the ultimate fibres, and these consisting either of hollow tubes, or strings of vesicles, or rhomboidal articulations, according to the respective theory with which the mind of the observer was prepossessed, it appears, from the best microscopical observations, that the fibres are divisible beyond what

fibres which possess the power of contracting themselves in virtue of their vital power, as distinguished from mechanical power, whatever their chemical elements may be.

the powers of the best assisted sight can trace, and that they are to all appearance uniform.*

This regular fibrous structure of muscles, though effected by powers peculiar to life, may be compared to the crystallisation of salts, and other regular forms which inanimate bodies assume, when passing to a solid form from a state of solution or fusion. Every species of matter has a mode of aggregation peculiar to itself, when its particles are at liberty to attract each other according to that tendency which has been called their *polarity*. Those who first conceived this idea, seemed to have proceeded on the supposition of the ultimate particles of matter being solid bodies, infinitely hard, having their different sides endowed with different powers of attraction and repulsion, so as to give various configurations to the parts of matter, when concreting into a solid form. There is a still more profound doctrine on this subject, founded on the hypothesis of the ultimate particles of matter being combinations of attracting and repelling points, which when brought much within the natural limits of these powers, produce unequal degrees of attraction and repulsion at equal distances from their common centre, thereby defining what may be called the shape of the particles, and constituting

* Some new light has been thrown upon this subject, since this Lecture was first published. See an article by Sir Everard Home in the Phil. Trans. for 1818.

polarity.* In whatever manner we conceive this to take place, some such circumstance seems universal, and perhaps necessary to all the varieties of solid matter; and there is in some instances a difference in the appearance and other properties of the same substance, after passing from a fluid to a solid form, according as its particles have been at liberty to follow more or less freely the tendency of their polarity in the act of concretion. This may be illustrated by the freezing of water, and the crystallisation of salts, which are more or less regular or confused, according to the circumstances in which they have taken place. The same may be exemplified in metals and other substances; for it is well known, that the properties of iron and glass, in point of cohesion and elasticity, are very much affected by the quickness or slowness with which they pass from a state of fusion to a state of solidity. It is probable in some circumstance of this kind that muscles differ from other soft animal matter. We cannot trace, by inspection, the manner in which the fluid nutritious matter is ultimately *applied* in forming solid parts; but as muscles are composed of parts so regularly figured and endowed with contractility, it seems probable that there is some provision made by Nature, whereby the particles follow the precise tendency of their

* See Dr. Blagden's Experiments on the cooling of Water below its freezing point. Phil. Trans. Vol. LXXVIII. page 143.

polarity, and constitute a more exquisite structure than in other parts of the body.

So far with regard to the character of a muscle, considered in its dead state.

The first circumstance that meets the attention in considering its living state, is that contractile power or motion, which is properly the subject of this Lecture; and in order to investigate its nature, it will be necessary to compare it with that which takes place in inanimate bodies, by considering the nature of motion in general.

So far as we know, either from actual observation, or from analogy, there does not exist in nature any such thing as absolute *rest*: for when we contemplate the motions of the earth and heavenly bodies, the various complications of the planetary revolutions in their rotation round their own axes, and in the paths of their orbits, in the irregularities arising from the disturbances of their mutual gravitation, and from the precession of the equinoxes, not to mention the influence of the innumerable sidereal systems upon each other,* it may be affirmed, on incontestible principles, that no particle of matter ever was, or will be, for two instants of time, in the same place, and that no particle of it ever has returned, or will return, to any one point of absolute space which it has ever formerly occupied. Whether

* See Dr. Herschel's Paper on the Construction of the Heavens. Phil. Trans. Vol. LXXV. page 231.

motion, therefore, can strictly be called an *essential* property of matter or not, it is, certainly, by the actual constitution of nature, originally and indefeasibly impressed upon it; and as rest does not exist in nature, but may be considered, in a vulgar sense, as a fallacy of the senses, and in a philosophical sense, as an abstraction of the mind, it follows, that what is called the *vis inertiae* of matter, is not a resistance to a change from rest to motion, or from motion to rest, but a resistance to acceleration or retardation, or to change of direction. If it should be alleged, that any given particle or portion of matter is carried along by virtue of the motion of the planet to which it belongs, it may be answered, that the earth or any other planet is nothing more than a congeries of such particles, each of which must possess a share of the same energy which animates the whole mass.

The active nature of matter is further proved by those attractions and repulsions which universally take place among its parts, however near or remote; and every instance of motion within the cognizance of our senses, in the bodies around us, is referable, either in itself or its cause, to some mode of attraction or repulsion. Mechanical impulse being the most familiar cause of motion in the ordinary events of life, is apt to be considered as the most simple and original cause of it; but it is obvious, upon reflection, that it

cannot originate in itself, and that all collisions are produced either by the efficiency of living animals, that is, by muscular action, or by means of some operation of nature, depending on attraction or repulsion. Of the first kind, all the mechanical operations of art are examples; and with regard to the others, they may, if carefully investigated, be referred in every instance, either immediately or remotely, to the above-mentioned inherent energies of matter. The natural agitation of air or water, for instance, may produce motion by impulse, or may bring two solid bodies to impinge upon each other; but it is evident that all such-like motions in the atmosphere or the ocean, could not take place without gravitation, which is one of the attractive powers of matter. From the familiarity of impulse in the common experience of life in external nature, we imagine that we have a more clear conception of it as a cause of motion, than we have of gravitation, electricity, or magnetism. The difficulty of conceiving these, consists in the apparent impossibility of a body acting where it is not actually present. But a little reflection will make it clear that the same difficulty exists with regard to impulse; for it is demonstrable from the phenomena of expansion and contraction of bodies by different degrees of temperature, that there can be no such thing in nature as the absolute contact of the ultimate particles of matter. It follows that in

the act of impulse, the particles of matter must act upon each other at a certain distance.

Attraction and repulsion may be considered as one principle, inasmuch as they are both expressive of that active state originally inherent in matter, and because any two particles acting upon each other, either attract or repel, according to their distance, their temperature, and affinities; and this is so universal an agent in nature, that some modern philosophers have made it absorb, as it were, every other power and property of matter. The late father Boscovich,* of Milan, about forty years ago, advanced a very bold doctrine to this effect, alleging with great strength of argument, illustrated by geometrical reasoning, that there does not exist in nature any such thing as impenetrable extended particles; and he deduces the phenomena of the material world from one principle, which supposes it constituted of points having several spheres of attraction and repulsion, which being variously arranged and combined, produce the different forms and properties of matter, and its several powers of attraction, whether chemical affinity, cohesion, or gravitation. Whether this hypothesis is founded in truth or not, it would appear from the reasonings made use of, that all the relative properties of

* See this doctrine fully explained, in a work entitled, *Theoria nova Philosophiæ naturalis redacta ad unicam legem*, &c. *Auctore Rogerio Boscovich. Venetiis, 1763.*

matter may be accounted for, though we abstract from every other consideration but attraction and repulsion.

It is evident, therefore, that whatever may be the cause of muscular motion, it is not referable to mechanism, which is itself only a secondary principle. Some theories have had recourse to the conveyance of a fluid into the fibres of muscles, by which they were swelled, and thereby shortened. One of the most plausible of these hypotheses supposes this fluid to be the blood; but this is plainly a *petitio principii*; for in order to give motion to the blood, the very power in question is necessary. Other fluids have been supposed to have this effect, but even the existence of these has not been proved. I will not detain this learned audience with a recital of the numerous theories of this kind that have been invented by fanciful and ingenious men, only *one* of which can be true, and the most solid objections could be urged against them all. Other arguments, derived from the nature of irritability and sensibility, could, if necessary, be brought to prove that muscular motion cannot depend on any mechanical cause: but this part of the subject was fully treated of by the ingenious gentleman who delivered the Croonian Lecture last year.

As it has been proved that all matter is in a state of perpetual motion, originally impressed upon it by Nature, also that attraction and re-

pulsion are essential to it, and the ultimate causes of all new motions that can arise in the universe, mechanical action being only a secondary cause, it seems most agreeable to the analogy of nature, to refer muscular motion to an original law of animated matter, whereby its particles are endowed with an attractive power for which no cause can be assigned, any more than for gravitation, cohesion, or chemical affinity. If I understand it aright, this was the doctrine laid down and illustrated last year by Dr. Fordyce, and to which I am endeavouring to contribute some additional proofs and illustrations, from a conviction, that it is the only rational and philosophical light in which this subject has hitherto been viewed.

The peculiarities of muscular attraction, as distinguished from every other instance of it in nature, shall be the first object of my research. If the shortening of a muscular fibre depends on this increased power of attraction between its particles, the effect of it will be to add to the power of cohesion in the fibre; and if this shall be found in fact to be the case, it will be a further proof of the doctrine just now advanced. In order to decide this, I made the following experiment upon the flexor muscle of the thumb of a man, five hours after death, while the parts were yet warm and flexible. All the parts of the joint having been separated, except the tendon, a

weight was hung to it, so as to act in the natural direction, and was increased gradually till the muscle broke, which happened when twenty-six pounds had been appended. I found that a man of the same age, and the same apparent size and strength, with the subject of the preceding experiment, could with ease lift thirty-eight pounds by the voluntary exertion of the same muscle. It is further in proof of this fact, that in the case of a violent strain from muscular contraction in the living body, it is the tendon that gives way, whereas we have seen, in the experiment just now related, that in the dead body, the muscle is the weaker of the two. It is also well known, that in cases of over-exertion, the muscular fibres themselves do not give way, though the strongest tendons, such as the *tendo Achillis*, and even bones, such as the knee-pan, are broke by their living force,* which, in such instances,

* There is a case related in the Philosophical Transactions, by Mr. Amyand, wherein the *os humeri* was broken by an exertion of the muscles, see Phil. Trans. Vol. XLIII. page 252. Every one has observed or heard of fractures happening from very slight accidents. These occur most probably from a jerk of the muscles concurring with the external violence. An accident in illustration of this happened a few days before this was written, (June, 1822): a nobleman about fifty-five years of age, by striking his foot against the foot of a skreen, was made to stumble and fall on his side. It was found that the *os femoris* was fractured, but not on the side on which he fell, so that the fracture could not be imputable to any external impulse,

must be many times greater than the strength of the dead fibres.

The sensible increase of hardness in a muscle, when in a state of contraction, may also be considered as a proof of an increased attraction of its particles to each other at that time.

In investigating this subject further, it is of importance to determine, whether or not a muscle, when in a state of contraction, undergoes any change of density. A comparison of it in this respect with dead matter, may throw some light on the nature of muscular action.

Every homogeneous body possesses a certain degree of density, determined by the distance of its integrant particles. The most common means in nature, by which the density of such bodies is altered, are heat and cold; the one universally producing expansion, the other condensation. Mechanical force has an effect in adding to the density of some species of matter, as is found in the hammering of iron; but it has not been ascertained by experiment, as far as I know, whether the forcible extension of bodies has any effect in diminishing their density. In the elongation of an elastic chord, it is evident that there must be an increased distance of the particles in one dimension, but as this may be compensated by a contrary change in the other dimension, the question purely to the muscular action, excited by the instinctive effort to save himself from falling.

tion regarding the change of density can be decided only by experiment. And as this is the point with regard to inanimate matter, which bears a seeming analogy to muscular motion, I thought it worth while to institute an experiment, to decide it. I took a piece of the elastic gum, or *caoutchouc*, three inches square, and about the eighth of an inch in thickness; I procured a piece of sheet-tin, three inches broad, and six inches long, cut into sharp teeth at each end. The gum was first weighed in air, and found to be 380.25 grains. It was then weighed in water, along with the tin, to which it was loosely attached, and the weight of both was then 758.75 grains. The gum was then stretched upon the tin, by means of the teeth at each end, to a surface of about five inches square, the tin being bent so as to leave a free space between it and the gum, in order that when immersed in the water, no air bubbles might be entangled. In this situation, the weight of both in water was found to be 746.75 grains. Here was a difference of twelve grains, which could be owing only to a diminution of specific gravity; and in order to be sure that there was no fallacy nor inaccuracy in the experiment, the gum was immediately after disengaged from one end of the tin, so as to allow it to shrink, and being again weighed in this state in the water, it was found to have recovered exactly its former weight. This, as well as the

subsequent statical experiments, was performed by means of the exquisite balance lately invented and constructed by Mr. Ramsden, and belonging to Sir Joseph Banks, who politely allowed me the use of it. I was also assisted by Mr. Gilpin, clerk of the Royal Society, who is extremely accurate and expert in all operations of this kind.

Now, does the state of relaxation and contraction make in like manner a temporary difference in the density of muscles? When the circumstance of *decurtation* only is considered, we should be tempted to think that there must be an approximation of the particles of the fibre; but there is at the same time a lateral swelling of the muscle, which may compensate for what is lost in the other dimension. This point cannot be decided but by an experimental examination. It might be determined whether a muscle occupies most space when relaxed or when contracted, by finding its specific gravity in each of these states by means of the hydrostatic balance. But this would be found extremely difficult; for the state of contraction is very transitory, and the motion itself would produce such a disturbance, as would render the result unsatisfactory. As there is this obstacle to the experiment on a living muscle, it occurred to me that it might be performed on the muscles of a fish, which had undergone the operation of *crimping*, as it is called; for, in consequence of dividing the muscles, by cutting them

when alive, they undergo a contraction, which continues after death;* and upon comparing, by the hydrostatic balance, portions of muscle which had been crimpt, with those of the opposite side of the same fish, which had on purpose been saved from this operation, it did not appear that there was any difference in the specific gravity. Two trials were made, one with the masseter muscles of a skate, the other with the sides of a large trout.

The following experiment was also made, in order to decide the comparative density of a contracted and relaxed muscle. I took a glass flask, into which one half of a living eel was introduced. The mouth was immediately afterwards fused by a blow-pipe, and drawn into a tube like the stem of a thermometer. The flask and tube were then

* It has been made a question, whether life, and its actions, may not affect the *absolute* gravity of bodies? Though this doubt has not arisen upon any assignable grounds that I know of, unless it be that one unknown principle may affect another equally unknown, I thought it might be worth while to determine it by experiment. The first trials were with animals of warm blood inclosed in oilskin, and close tin vessels; but not being satisfied with the accuracy of these, from the difficulty of cutting off all communication with the external air, so as to prevent moisture from exhaling, I inclosed live eels in flasks, and sealed them hermetically; and, in this situation, their weight when alive being compared with their weight when dead, there did not appear any reason to suspect that the mere circumstance of life made any difference in regard to gravity.

filled with water, in order to see whether the motion of the animal would make the fluid rise or fall. It had neither the one effect nor the other, though there were at times strong convulsions, and if the muscles had at any one time occupied either more space or less than at another, a sensible fluctuation would have been produced, especially when the column of fluid was rendered very fine, by the introduction of a steel wire to irritate the parts. That part of the eel from the *anus* to the tail was made use of for this experiment, as the other division, containing the organs of respiration and the air-bladder, might have occasioned a fallacy, from the expansion or condensation of an elastic fluid, by accidental changes of temperature, or compression. This was repeated three times, with the same result. In one of the trials, the above mentioned portion of two eels was introduced, and though they were at times both in convulsions at once, not the least motion of the fluid in the tube could be perceived.

I was the more desirous to be accurate in this and the preceding experiments, as the result of them was different from my own preconception at the time, and different, I believe, from the opinion of most modern physiologists. It may safely be inferred from them, that the contraction of a muscle produces no change in its density, and that animal life differs from inanimate matter in this respect, as well as in most of its other

properties and laws. One purpose in nature for muscles always preserving the same density may be, that as some of them act in confined cavities, inconvenience might arise from their occupying more space at one time than another. In the extremities of crustaceous animals, for instance, which are filled with muscles, a change of density could not fail to burst them. This may also be considered as a proof of the fact itself.

Another circumstance in which the contraction of muscles differs from simple elasticity is, that the former, however frequent and violent, does not produce any heat, as collision and tension are known to do. I found that a very sensible degree of heat was produced by the quickly repeated extension and relaxation of a piece of elastic gum ; but it is not found that the like repetition of action in a living muscle has the same effect. This may admit of some cavil with regard to animals of warm blood ; for, one of the theories with regard to animal heat is, that it arises from the perpetual vibration of muscular fibres, particularly those of the vascular system ; but this will not hold with respect to animals of cold blood, in which the actions of life are equally vigorous.

The principal phænomena, therefore, of muscular motion, are the shortening of the fibres, the lateral swell, the increase of cohesion and hardness, and the unchanged density and temperature. It would appear, from the two last circum-

stances, that the intimate motions of the particles in relation to each other, must be different from what takes place in the several instances of contraction and expansion in inanimate matter. In the expansion arising from the action of heat, and the contraction from cold, the change of density shews that in the one case, the ultimate particles must recede from each other; and in the other case, that they must approach. The same may be said of elasticity. But as there is no alteration of the density of a muscle in passing from relaxation to contraction, this change cannot consist in the approximation of the integrant parts of the fibres, but must depend on some other circumstance in the intimate disposition of the particles. In attempting to conceive in what this consists, the following explanation may be offered: It was formerly mentioned that the regular structure of solid bodies depended on the polarity and shape of their integrant parts. Now, all bodies, except such as are spherical, must have a long and a short axis; and let us imagine the fibres of muscles to be composed of sphæroidal particles; we may then conceive relaxation to consist in their being disposed with their long axis in the line of the fibres, and contraction to consist in their short axis being disposed more or less in that direction. This will not only account for the decurtation, and uniform density, but for the lateral swell, and also for the increased hardness

and cohesion; for though the particles do not approach or recede, as in bodies simply elastic, yet their power of attraction will be increased by their centres being brought nearer, and by being applied to each other by more oblate surfaces. This hypothesis accords with what has been before proved, concerning the unchangeable density; for what is lost in one dimension, is gained in another; and the cause for there being no increase of temperature, depends probably on the same circumstance by which the density is preserved unaltered.

WHAT has been hitherto advanced on this subject, has tended only to explain that state of a muscular fibre which renders it susceptible of contraction, and to ascertain the nature of that change which takes place in its passing from the state of relaxation to that of contraction. It still remains a question by what efficient power this contraction is excited. We have, indeed, referred the cause to attraction; but of attractions some are perpetual, such as gravitation, which exerts an equal and unremitting *nisus* upon every particle of matter, which is the subject of its action; and there are others fugitive and occasional, such as electricity and magnetism, and we may add muscular contraction. With regard to the first kind, as it is always uniform, it seems sufficient

to say, that it exists as a part of the invariable constitution of nature ; but with regard to those which are fluctuating, it seems incumbent on those who search into the laws of nature, to say by what mode of efficiency the attraction is performed, so that its action should take place at one time, and not at another. In order to answer this question, with regard to muscular motion, we ought to be able to specify by what mode of operation a *stimulus* excites contraction. Those theories which account for the contraction of muscles, by the swelling of the fibres, in consequence of a conveyance of matter, professed to account for the manner in which *stimuli* operate. But upon the principles I have adopted in this lecture, I am obliged to confess my entire ignorance on this subject. Perhaps it is inscrutable. Perhaps the state of human knowledge is not ripe for such an inquiry, for we are still in the dark with regard to most of those properties of matter which bear any analogy to this, and the knowledge of which might tend to throw light upon it. All that department of philosophy which relates to the corpuscular changes of matter, and the influence excited upon them by what have been called the *imponderable* fluids, that is, heat, light, electricity, and magnetism, to which may be added gravitation, is extremely obscure and unsettled. It is not even decided whether they are really *matter*, or only certain modes of action.

We ought not to wonder then that the equally subtle cause of muscular motion should be equally dark and unfathomable ; and as the most chaste and correct philosophers have agreed to assume the former as ultimate principles of nature, let us do the like with regard to that animal attraction in which muscular contraction consists. And the exciting causes of these motions called *stimuli*, being equally inscrutable as to their mode of action, let us restrict our attention to what is palpable and practical, namely, their history and results.

In prosecuting this history, the first thing that naturally occurs to be done is to enumerate the *stimuli*.—They may be divided into internal and external. As an example of the former, the circulation of the blood may be mentioned ; as this is kept up by an exciting influence of the blood upon the heart and vessels which contain and impel it. The earliest perceivable instance of muscular motion, is the beating of the heart, as it is seen in the first rudiments of the embryo in an egg, and called the *punctum saliens*. There seems to be established by nature a certain *habitude* of action between the vessels and their fluids, whereby the former are duly stimulated to propel the latter. This does not depend merely on the acrimony of the fluids ; for if a fluid even more mild than the blood, such as milk, be injected into the circulation, it will produce great dis-

turbance; and the property imparted to the blood, by oxygen through respiration, whereby it supports life by stimulating the heart, is of a specific nature, and distinct from simple acrimony.

The irritability of all the containing parts, is in like manner accommodated to the nature of their respective contents. The intestines are so calculated, as to have proper motions excited in them by the aliment, and the secretions which are mixed with it; and there are bodies which, though perfectly mild, such as alimentary substances of difficult digestion, yet excite more violent commotions in the stomach than other substances which are very acrimonious. The various effects of poisons in different parts of the body, may also be mentioned as an illustration of the susceptibility peculiar to the several organs of the body. The poison of a viper, for instance, is perfectly innocent, not only in the receptacles of the animal which produces it, but it may be taken into the stomach of any animal without the least bad effect, and only exerts its deleterious power when brought in contact with a wounded part. Some vegetable poisons, on the contrary, such as that of laurel water,* prove deadly, when taken into the mouth, or applied to any part of the alimentary canal, but are innocent when injected into the veins. The same principle might be illus-

* See Experiments on Poisons, by Abbé Fontana.

trated by the operation of various medicines, some of which act upon one set of organs, and some upon another. But it is meant here more particularly to elucidate the natural internal actions of the body ; and it may be remarked, that the receptacles of the several secreted fluids, such as the gall bladder, and bladder of urine, are so adapted to their natural contents, by a due measure of irritability, as to bear their accumulation to a certain degree, and then to expel them. We have here also a proof that irritability is not in proportion to sensibility ; for both these receptacles are extremely sensible to pain and irritation, from extraneous acrimony, though so moderately sensible to the acrimony of their natural contents. This disposition in the several organs to perform their natural functions, in consequence of the *stimulus* of the respective fluids they contain, has aptly enough been called the specific *perception* of these organs.*

It follows from this, that the application of chemical and mechanical *stimuli* to irritable parts, is not a mode of experiment likely to be productive of useful knowledge, since the internal organs are calculated to perform their actions in consequence of peculiar and specific *stimuli*,

* This idea is well illustrated by Mr. Hunter in his lectures ; also by Mr. Mudge, in a dissertation on the *vis vitæ*, subjoined to a tract, entitled, “ A radical and expeditious cure for a catarrhus cough.”

provided by nature ; and this consideration may serve to suggest the most likely means of restoring lost irritability and action to the vital functions, when suspended by suffocation, strangulation, or submersion. The action of the heart depends on the action of the lungs and the inspiration of atmospheric air ; and I have found, from repeated experiments on animals, that in such cases, all other means of restoring circulation and life, are of little or no avail, in comparison of inflating the lungs with atmospheric air, and by stroking and pressing the ribs, so as to imitate the action of respiration. It is singular that this should have escaped the attention of those who have drawn up directions for the restoration of drowned persons. Neither mechanical friction, nor any other external *stimulus*, nor stimulating clysters, seem to have any sensible effect in recalling life. The only other means, besides those above mentioned, that seem material in attempting to restore suspended animation, is a due attention to the external temperature. In the case of drowning in cold water, for instance, it is of the utmost consequence to restore the natural warmth, either by the cautious use of artificial heat, or the application of living bodies. In the case of those who have been suffocated by unwholesome vapour, it is, on the contrary, advisable to expose them to cool air, and to dash cold water upon them.

From what has been before advanced, concerning this habitude or mutual influence of the

solids and fluids, it would appear that they are specifically appropriated to each other, in order to carry on not only the circulation, but the important functions of digestion, absorption, secretion, and excretion; and as the healthy condition of an animal consists in the maintenance of that natural harmony, so must the state of disease depend on the derangement of those delicate impressions and nice sensibilities, or rather *irritabilities*, in which the functions of the several organs consist; and as the affections of the solids and fluids are reciprocal, disease may depend either on some deviation of the former from their healthy and natural perceptions, or from some acrimony or vitiation of the latter, or perhaps more commonly from the concurrence of both, in consequence of their mutual influence. But as the vitiation of the fluids is referable to the morbid action of the solids, the specific qualities of all the fluids being determined by the vital action of the secretory organs, the solids must be the original seat and cause of disease, and remedies intended to produce a radical effect must be addressed to them. Nevertheless, as many of the symptoms of disease depend on the acrimony and vitiated state of the fluids producing considerable suffering and disturbance, one of the principal objects of practice will consist in the elimination of them.*

* See this subject more fully treated in Elements of Medical Logick.

It is evident, that this doctrine will admit of a much more extensive application in pathology, than there is time here to follow out ; and I shall confine myself to the illustration of it, in the case of the absorbent vessels. These evidently possess a power of absorbing certain substances, and rejecting others. The lacteals, for instance, in a state of health, take up only the nutritious part of the alimentary mass ; for there is in the fecal part, substances equally soluble as the chyle ; and this fecal part is actually absorbed, when the lacteals are in a preternatural state, whether from disease or from the action of mercury, as is discoverable by the fetor of the breath. Indeed, the operation of mercury as a medicine, seems to consist in that preternatural absorption excited by it, whereby it removes syphilitic matter and nodes. In like manner, the inner surface of the gall bladder is beset with absorbents, which, however, do not absorb bile in the ordinary state of health, and only concentrate it by taking up the fluid with which it is diluted. But when, in consequence of the obstruction of the gall ducts, the bladder becomes over distended, or when the specific perception of the absorbents is depraved by disease, in these cases the bile is absorbed and thrown into the circulation. At other times, disease consists in affections of these vessels, either by their action being too much retarded, as in the case of dropsy, or where the matter of an

ulcer, or that of the pustules of the small pox, is prematurely absorbed, in consequence of the depraved action of fever. It is sufficiently demonstrable, that the whole surface of the skin and *bronchiæ* is beset with inhaling vessels, which absorb the fluids dissolved in the atmosphere; and it would be contrary to the analogy of the rest of the body, to suppose that these are not possessed of some elective power, whereby they prefer or reject such fluids as are presented to them, according to their several qualities, and that this power should not be various, according to the state of health or disease. But, independent of analogy, the variable state of the human body, in respect to its susceptibility of contagious diseases, seems to be a direct proof of this. This has already been illustrated in the Dissertation on Infection.

The specific irritability of muscular fibres, in consequence of the peculiar action of *stimuli*, has been called *perception*, as was mentioned before. This term is not to be taken in a sense strictly literal, but as a metaphor, borrowed from sensation, and applied to motion. In like manner as the senses are fitted to convey peculiar ideas, in consequence of their respective organs being adapted to their corresponding external impressions, so are the various organs of motion by nature made susceptible of excitement from peculiar impressions, either internal or external.

But though there can be no doubt of the reality and necessity of nervous power for the purposes of sensation and volition, it is very questionable how far it is necessary to the involuntary actions, those for example which carry on the circulation. The organs subservient to the latter are certainly supplied with nerves, and the irritability of the heart was supposed to be dependent on the nerves with which it is supplied. This seemed to be confirmed by the action of the heart being suspended by the destruction of the brain, or the division of the nerves leading to these parts ; but it has been found by Bichât, and others, that this arose from the nervous power subservient to respiration being interrupted, and that it could act, though its own nerves were divided, provided respiration were maintained.

It is in these points that the chief physiological controversies have arisen. Having elsewhere* endeavoured to prove that the main cause of these contests has been the want of discriminating between those actions of which the nervous system is the *constituent* and *actuating* cause, from those in which it is only *influential* for the purpose of maintaining the connexion or inter-nuntial correspondence, as it were, of one organ and function with another, as is necessary in the more complicated animals, I need not here enlarge on this subject.

* Elements of Medical Logick.

The question, however, concerning the existence of irritability, as well as other attributes of life, independent of the nervous system, is an essential part of our subject. Mr. Hunter maintained that in the most simple animals no brain or nerves could be found, and that they are unnecessary ; for, these animals possessing no function but that of assimilation, they do not require that variety of action, and that complicated influence necessary for the mutual actions, reactions, and sympathies of the compound animals of the superior orders. It is well known, not only that the muscular fibres of animals endowed with a nervous system will retain their irritability for some time after their separation from the brain and nerves, but that there have been monsters destitute of brain, and even of spinal marrow. It is not a little in favour of this opinion also, that no nerves are to be found in the placenta.

There are, besides irritability, other attributes essential and peculiar to life evidently independent of the nervous system, as may be exemplified with regard to that conservative principle by which living animal substance counteracts putrefaction, and also, that by which it generates and maintains heat. It is evident, likewise, from the *phænomena* of vegetation, that irritability may exist in nature, without sensation, consciousness, or any suspicion of the existence of a nervous system. The facts I allude to, are not only the

perceptible motions of the sensitive plant, but more particularly those motions which must necessarily take place in all plants, in carrying on their growth ; for there is no accounting for the accretion of solid parts, in consequence of the conveyance of nutrition by the propulsion of the sap, but by admitting some power, acting by laws different from those of dead matter. How could the matter which composes the wood and foliage of the summit of a tree have arrived there without a vital power counteracting gravitation? Lastly, the state of an egg before incubation, or of a seed under circumstances in which it is not excited to developement, also the condition of those animals which become torpid from cold, and afterwards revive, are in proof of this ; as they shew that there is a certain principle of self-preservation, independent not only of the operation of the nervous system, but even of organization and of the circulation ; for, in this quiescent state, these portions of organic matter are preserved for a great length of time from that corruption to which they would otherwise be liable, and their fluids are prevented from freezing in a degree of cold, which would congeal them, were they destitute of every principle of life, and in other particulars, form exceptions to the establishment of that equilibrium of temperature which takes place in inanimate matter.

But though simple life may be considered as

distinct from the nervous system, which is only an accessory appendage to it, yet in those animals in which they are conjoined, the purposes of nature render them dependent on each other. The functions of the brain, for instance, cannot go on without the action of the heart; for whenever the circulation of the blood is interrupted, consciousness and sensation are destroyed, as is evident in the case of a swoon, and in the effects of strangulation. On the other hand, as has been before observed, the action of the heart has a dependence, though indirect, on the influence of the nervous system. There are also incontestable proofs of the extreme vessels being affected by the influence of the brain; for we know that a thought in the mind will produce partial determinations of the circulating fluids, as in the case of blushing, and the fulness of the vessels in the organs of generation and digestion, in consequence of their respective appetites. It does not, however, follow, from all this, that irritability depends on the nerves; the influence of which may be considered as modifying general irritability, in the manner already mentioned; or it may be considered rather as a *stimulus* to the muscular fibres, than as endowing them with irritability, as in the instances last adduced. We have seen that the heart can act independent of the nervous system, and that the vessels of the extremities can exert their usual action independent of it; for

there are cases in which the natural heat and circulation continue in the limbs, after a total deprivation both of voluntary motion and sensation. This fact is ascertained, both by the experiment of cutting the crural nerve of a living animal, and by the circumstances attending certain diseases. I lately met with two cases of palsy, in which there was a total loss both of sensation and voluntary motion in the lower extremities, and yet the natural warmth and circulation remained. In one of these cases, excoriations were produced on the feet by sinapisms; and in the other, blisters rose on the knees, but without exciting any sensation, and the parts healed as in a healthy person. The first, was that of a gentleman advanced in life, in whom this affection came on after the gout in the stomach, and he died in consequence of the palsy extending to the bladder and other *viscera*. The other was that of a young woman in St. Thomas's Hospital, who had been subject to violent hysterical convulsions. After a tedious illness, she entirely recovered the use and feeling of her limbs. Mr. Edwards, at Paris, has found that a wound in the extremity of a frog will cicatrise after the head has been cut off,* and Dr. Wilson Philip has found the same after the nerve of the limb has been divided. Even after the natural heat has left a muscle it is excitable by galvanism. This is the state of it so well described by Mr. Hunter in his Treatise on

* See Journal of Science, vol. iv.

the Blood and Gun-shot Wounds indicated by a state of *tension* and *firmness*, as distinguished from the state of complete death indicated by the *flaccidity* of the fibres.

But there are other circumstances that would seem to prove, that the nervous system is not only a mere appendage to life, but that it tends to impede its operation, and shorten its existence. Simple life will not only survive sensation, but will survive it longer, if the animal is killed, by destroying the nervous system, than if it had been destroyed by hæmorrhage, suffocation, or other violence. It is a curious and well ascertained fact, that if a fish, immediately upon being taken out of the water, be stunned by a violent blow on the head, or by having the head crushed, the irritability and sweetness of the muscles will be preserved much longer, than if it had been allowed to die with the organs of sense entire. This is so well known to fishermen, that they put it in practice, in order to make them longer susceptible of the operation called *crimping*. A salmon is one of the fish least tenacious of life, insomuch, that it will lose all signs of life in less than half an hour after it is taken out of the water, if suffered to die without any further injury; but if, immediately after being caught, it receives a violent blow on the head, the muscles will shew visible irritability for more than twelve hours afterwards.

There is a circumstance observed with regard

to animals of warm blood, which seems to depend on the same principle. An excessive exertion of voluntary motion, immediately before death, prevents the muscles from becoming rigid when cold, and renders them more prone to putrefaction. Thus, if an ox be killed immediately after being overdriven, the carcase will not become stiff when it grows cold, nor is it capable of being preserved by means of salt.

In illustration of the same principle, it may be remarked, that there is a symptom in certain diseases of the human species, shewing that digestion, which is one of the principal functions of simple life, will sometimes go on better, in consequence of lesions of the brain ; for in those disorders in which the exercise of the senses is in a great measure destroyed, or suspended, as in the hydrocephalus and apoplectic palsy, it happens not uncommonly that the appetite and digestion are better than in health.

From these facts we may infer, with Mr. Hunter, that the exercise of sensation is inimical to life, and that a sort of fatigue is induced by this, as well as by voluntary motion, in that complex animal machinery in which a nervous system is indispensable, so that all that intercourse carried on through the nerves, whether *towards* the brain, in the case of sensation, or *from* the brain, in acts of volition, tends to wear out the animal powers. And as intense and long continued thought,

though not terminating in any outward action, tends also to produce an inability for further exertions, it would appear that the brain, or sensorium, is more particularly the organ which is subject to that species of sufferance called fatigue. From these facts, we perceive the necessity of sleep, which consists in a temporary suspension of sensation, volition and thought, and is a resource of nature, whereby the powers of life recover themselves after satiety and exertion, which are provided as guards to warn us when nature is in danger of being strained, either by repletion or excess of action; and it is evident that such barriers were absolutely necessary, in order to set bounds to operations which are only occasionally requisite, and which would otherwise depend on the caprices of the will. The exercise of sensation and voluntary motion, in a moderate degree, is conformable to the intention of nature, and therefore salutary; and it is only when they are excessive, that they tend to wear out the powers of life, and more especially if these are not duly recruited by sleep. Immoderate labour, therefore, and watching, also spasms and convulsions of every kind, are unfriendly to health and long life: in like manner, sensations, when too frequent or intense, especially those which consist in the gratification of the senses, tend to wear out the animal powers; and hence we perceive why a life of sensuality is productive of certain diseases,

independent either of the repletion or evacuation which attends them. The gout, but more certainly the palsy, seems to proceed merely from the indulgence of the senses ; for the latter commonly enough occurs in the most spare and emaciated constitutions, and in those who have been accustomed to exhausting pleasures, as well as those of a full habit, who have indulged in the excesses of the table. A turgescence of the vessels in the brain will certainly be more apt to produce that rupture of them in which apoplectic palsy consists, when these vessels have been relaxed, as we conceive them to be, by frequent and intense sensations. But in those who are the reverse of being plethoric, and who fall victims to this disease, in consequence of too free indulgence in venereal pleasures, in the decline of life, (as every one who has much observation of the world, or experience in physic, knows to be a frequent case) it must arise from a preternatural weakness in the brain, induced by sensual excitement as much as by seminal exhaustion. This accounts for what has been reckoned a difficulty in reasoning on the cause of apoplexy and palsy, to wit, that the same effect should be produced by those gratifications which produce repletion, as by those which produce evacuation. It is observable, however, that these complaints are not uncommon also among the lower orders, in consequence of hard labour and privations.

It follows, from the same principle, that when life is threatened by certain diseases, of which the chief symptom is irritation, any means by which sensation, whether natural or morbid, and muscular motion, whether voluntary or involuntary, convulsive or spasmodic, can be soothed or suspended, will prove salutary, by allowing the powers of life to rally, as it were, and recover themselves. In this consists the operation of narcotic medicines, such as opium, which, in complaints both of a general and local nature, proves useful not merely as a palliative, by the removal of temporary pain or spasm, or by procuring sleep, but as a principal instrument of recovery, by allowing the restorative powers of life to exert their natural action, in consequence of the removal of irritation.

As an example of the general affections of the constitution in which opium is a useful remedy, we may mention those low fevers in which the principal symptoms are tremors, *pervigilium*, and low *delirium*. And as an instance of local affections, in which it has been found highly serviceable, we may mention ill-conditioned ulcers of all kinds, but particularly those which occur in the venereal disease. One of the principal difficulties in the cure of this disease, is that irritability of constitution whereby ulcers are so exasperated, by the use of mercury, as not to bear a sufficient quantity of it to produce a cure. This

is obviated by a free use of opium, which seems more efficacious in such cases, than even Peruvian bark, or any other remedy; and this is one of the principal modern improvements in the treatment of this disease.

Having considered the various qualities of the fluids exciting the corresponding irritability of the respective vessels, as a leading principle in carrying on some of the most important functions of the body, and serving to account for many of the vital and involuntary motions, the only other internal *stimuli* that remain to be enumerated, are those connected with consciousness. The great masses of muscle in the trunk and extremities of the body, are the instruments of the mind in acting upon external bodies; and we may, therefore, reckon in the list of *stimuli*, the nervous power by which the will and the passions excite external motions. This is a function sufficiently important for the nerves, without admitting them as the principle upon which irritability depends. This question has been already discussed; but it may be further observed, that the nervous power being a stimulus acting upon an irritable principle in the muscular fibres, affords a presumption that they are different from each other; for, the matter being considered metaphysically, where any effect is the result of the concurrence of two bodies, as, for instance, in the combinations of chemistry, these two bodies

must be different, in order to produce any overt effect. A *tertium quid* necessarily implies two terms of different properties. The motion from the impact of two bodies of the same nature seems an exception to this; but the change produced here is mere communication of the same action, not a new action, such as results from the concurrence of two heterogeneous portions of matter. From the concurrence of a particle of alkali with another particle of alkali, no result follows; nor from the concurrence of a particle of acid with another particle of acid. But let a particle of alkali meet a particle of acid, a result is effected. It is quite otherwise, in the case of mechanical impulse, where the same motion that is lost by one body is acquired by another. But it will not be said, that there is any thing in common, far less identical, in the nature of a nerve and muscle, nor that the act of voluntary contraction is a communication of motion from the one to the other.* It is hoped that this will

* I derived the ideas contained in this paragraph from a train of thought, into which I was led in an early part of my life, when I addicted myself to metaphysical studies, particularly to a question then much agitated, regarding the existence of matter; and perhaps the reader will excuse my transcribing here one of the notes which I then made on that subject. "It appears to me that the paradox of Berkeley may be refuted in two ways. First, it may be met by that argument called a *Reductio ad absurdum*; for if any position either leads in its inferences to a contradiction, or is irreconcilable to matter of

decide the controversy between Whytt and Haller. The *vis insita* of the latter seems to be the sound opinion on this subject.

I have already acknowledged my ignorance of the manner in which *stimuli* in general operate, and that this must be admitted as an ultimate fact in nature. But the operation of the will through the nerves, seems involved in double obscurity; for as it depends on the nature of thought, it cannot be made a subject of experimental investigation. For this reason I shall decline the inquiry, as not being adapted to the

fact, we are sure it is false, and that some vicious logick must have crept in at some step of the reasoning, though we may not have been able, in any other way to detect it. 2ndly, Let us ask, whence we derive the idea, or if you please, the word EXISTENCE? It may be answered, from our consciousness, and that we can have no certainty of any other existence but self-existence. But no single existence can effect any change or event, and a change or event of some kind there must be in order to create those sensations or states of mind in which consciousness consists. There must therefore be something in existence foreign to ourselves, for no change, in other words, nothing which stands in the relation of cause and effect, is conceivable, but what is the result of two existences acting upon each other. Let any one run over in his mind the various incidents that have occurred in the course of his experience, with regard to objects, whether animate or inanimate, and ask himself whether in every change there must not be a congress of two existences. It seem contrary to the nature of things that a solitary existence can be susceptible of a change, without the concurrence of something else."

ends of this Society ; and it seems impossible for human sagacity to penetrate the connection of matter with sensation and volition. All such attempts have consisted of abortive and unsatisfactory inferences drawn from hypothetical assumptions. The properties of different bodies, in relation to each other, appear to be the only proper subjects of experimental reasoning ; for, in their relation to the mind, they are only the effects, perhaps the remote effects, of their intimate nature upon the senses ; and we may venture to affirm that human reason can no more fathom the connection of thought with the corresponding changes in the corporeal organs, than the eye can see itself.

Those affections of the muscular fibres, which depend on the *passions*, though distinct from those excited by the will, may yet be enumerated here among those which flow from consciousness ; for there are emotions of the mind that have visible and powerful effects on the heart and vascular system, which are organs entirely out of reach of the will. Not to mention the well known effects of grief, fear, and joy, which affect the whole circulation, there are certain passions and sentiments which produce partial and local effects. These are established by Nature, either to answer some important natural purpose, as in the case of the congestion of the fluids in certain organs, as already adverted to, or to serve as natural expres-

sions, as in the case of blushing and weeping. One of the most striking effects of the passions upon muscular action, is the influence they have upon the strength or mechanical force of the voluntary muscles. Fear produces debility, almost amounting to palsy. Courage and ardor of mind, on the contrary, adds to the natural strength. When the mind is agitated by some interesting object, and calls upon the body for some extraordinary exertion to effect its end, the muscles are thereby enabled, is it were by magic, to perform acts of strength, of which they would be entirely incapable in cold blood. In circumstances of danger, for instance, where life or honour, are at stake, exertions are made in overcoming mechanical resistance, which seem incredible, and would be impossible, were not the mind in a sort of phrenzy; and it is truly admirable in the œconomy of nature, that an idea in the mind should thus in a moment augment the powers of motion and inspire additional resources of strength, adequate to the occasional calls of life.*

* This extraordinary degree of strength, infused into the muscles by ardent passions and affections, has been considered, by unenlightened minds and heated imaginations, as a *supernatural* influence; and the striking effects described above, may form some excuse for superstition in attributing them to the secret agency of some propitious and irresistible power. The etymology of *enthusiasm*, a word expressive of these uncommon exertions, shews that there was originally supposed to be, on certain occasions, some divine influence actuating the human

The great increase of strength in maniacs, is also referable to the passions of the mind. These considerations would almost lead us to doubt whether or not the accounts we have of the great feats of strength ascribed to individuals in the heroic ages, are fabulous or not. It is also worthy of remark, that in great and lasting exertions of strength, to which men are impelled by active and generous affections, fatigue is not induced in the same proportion, by many degrees, as by the same quantity of muscular action in the cool and deliberate actions of common life.*

2. The other class of *stimuli* to be enumerated, are the external. These consist in impressions made by outward bodies. They are either immediate, as in the case of those motions which are excited whether by mechanical means, or by acrimony, directly and artificially applied to a muscular fibre; or they are remote, as in the various instances of sympathy, and in the case of those instincts which nature has implanted for the purpose of self-preservation in brutes, and in the early part of human life. I shall here confine myself to a few remarks on instinct, as the other

frame. The consciousness of this increased vigor of mind and body, exalted by the belief of its divine source, will serve to account for those peculiar and astonishing efforts of enthusiasm, which are met with in the history of mankind.

* See Observations on the Diseases of Seamen, Book II. chap. III.

branches of this subject have been fully and ably handled by those who have gone before me in this Lecture.

There is a connection established between the impression of certain external bodies and the action of certain muscles, analogous to what has already been noticed with regard to the internal motions excited in vessels by the peculiar *stimulus* of their fluids, Nature having instituted certain habitudes between outward *stimuli* and the moving powers, whereby natural propensities are established equally necessary to the support of life as the internal functions. Thus, in a new-born animal, the first contact of the external air excites the act of respiration, and the contact of the nipple excites the act of sucking; both of which actions are absolutely necessary to the maintenance of life; and require the nice co-operation of a great number of muscles, prior to all experience. Actions of this kind are called instinctive, and differ from voluntary motions in this respect, that the latter are the result of memory and experience, whereas the former are the immediate effect of external impressions, in consequence of an established law of nature, and independent of consciousness. The actions of instinct and those of volition, nevertheless, run imperceptibly into each other, so that what was at first instinctive, may afterwards come to be a matter of deliberate choice. The same muscles

are the instruments of both, and they differ from the muscles obeying the internal *stimuli*, such as the heart, in this respect, that they are liable to fatigue, and thereby concur with the exercise of sensation and of thought, in rendering sleep necessary. There are no muscles, except those of respiration, of which the constant action is necessary to life, and which are void of consciousness in their ordinary exercise, but which are occasionally under the control of the will. The principal end answered by this power of the will over the muscles of respiration in man, is to form and regulate the voice.

But though instinctive motions are in some cases convertible into those which are voluntary, we should be so far from confounding them, that the former are even compatible with the want of consciousness and sensation; for those animals which are destitute of brain and nerves, are capable of actions analogous to the instinctive. There is something very similar to this even in vegetables, as in the case of tendrils and creeping plants being stimulated, by the contact of other bodies, to cling round them in a particular direction. There are facts, which shew that instinctive actions, even in animals endowed with brain and nerves, do not depend on sensation. I took a live kitten, a few days old, and divided the spinal marrow, by cutting it across the neck. The hind paws being then irritated by pricking them, and

by touching them with a hot wire, the muscles belonging to the posterior extremities were thrown into contraction, so as to produce the motion of shrinking from the injury. The same effects were observed in another kitten, after the head was entirely separated from the body. In repeating this experiment, I found that when the spinal marrow was cut through, between the *lumbar vertebræ* and *os sacrum*, the posterior extremities lost their irritability, but the part below it, the tail, retained it. It might, therefore, be said, that the spinal marrow below the division, served as a sensorium; but it may be answered, that when the head is cut off, its irritability remains, as appears by the motion of the ears, when pricked or touched with a hot wire; and as the extremities are also irritable, it will not be said that consciousness and sensation exist in two separated portions of the same body. Nor can it be admitted that sensibility and consciousness may remain in the head after separation; for, if mere compression of the carotid arteries abolishes sensation and thought, by interrupting the circulation in the brain, how much more must the superior violence of decapitation have this effect? In an acephalous monster, the like *phenomena* were observable. It moved up its knees when the soles of the feet were tickled: it performed the act of suction; passed urine and feces, and swallowed food. It is on record that the same took place in the case of

one, in which the spinal marrow, as well as the brain, was wanting. The like takes place with regard to insects; for, after the head of a bee is separated from the body, the hinder part will sting, upon the application of such a *stimulus* as would excite the same action in the animal in a perfect state. These facts show clearly that instinctive, or rather automatic motions may be exerted, without the intervention of the *sensorium commune*, and therefore, without sensation or consciousness.*

Before quitting this subject, it is of some importance to advert to a curious and interesting connection of voluntary motion and sensation. It is observable that pain excited in any extremity of the body, impairs, for the time, the vigour of the neighbouring muscles, though these have not been the seat of the injury. For example, a smart blow on the shin, will make a person limp, and render him almost incapable of supporting himself on that limb. On the other hand, muscular action brings alleviation to severe pain, as is observable from the writhings and groanings of those under the smart of bodily suffering. If we were disposed to admit loose theories, we should say that the physical cause of these phenomena was the resistance given by the opposite currents

* See a case by Dr. Yelloly in Medical and Chirurgical Transactions, vol. iii., with reference to other cases in proof of the automatic motions being independent of sensation.

of the nervous fluids belonging respectively to volition and sensation, in meeting each other on the route to and from the sensorium, and the extreme parts.

IN what I have further to say on this subject, I shall confine myself to the consideration of two of the most curious and important attributes of life belonging to muscular action, namely, HABIT and IMITATION.

It is the nature of a voluntary muscle to perform any motion with greater ease, the more frequently it is repeated, and to act most readily with those muscles, or in company with those sensations with which it has been used to combine its action, either at once or in succession. This is the foundation of habit, and is the principle by which all the practical attainments of man acquire facility and perfection. It has been mentioned that some actions, originally instinctive, may afterwards be performed as acts of pure volition; so inversely, all actions, which are the result of reason and reflection, may be brought by habit to resemble instinctive actions, and thereby to be performed with greater expedition and effect.

The term Habit has also been applied to sensation; for, as motions are more readily excited by frequent succession, so one perception excites the idea of another, in consequence of repeated con-

nection. In this sense, it ought more properly to be called the association of ideas, a principle upon which Dr. Hartley has built his theory* of the human mind, which, however exceptionable in other respects, is thus far conformable to Nature. It is habit, taken in this sense also, which Mr. Hume† conceives to be the foundation of all our experimental reasoning, inasmuch as it constitutes the only means by which we acquire any intimation of the connection of cause and effect. But though this doctrine is ably and profoundly illustrated by that philosopher, it may be remarked, independently of other objections, that though these habits are indeed by the constitution of our nature indispensable for acquiring useful and practical habits, they could not have been so unless there had been a corresponding constitution of external nature, without which animals, whether rational or irrational, would have been made susceptible of habit in vain, just as the eye would have been made in vain, had there not existed such a thing as light. Is it not, therefore, a mere abuse of words to say that habit is the sole constituent of all our experience? This constitution of our senses and faculties by which they are rendered correspondent, or

* This work has been republished, with a preface, by Dr. Priestley.

† See *Essays and Treatises on various Subjects*, vol. iii. by David Hume, Esq.

parallel as it were to the properties of the external world, consists in that principle whereby nature acts by invariable laws ; for it is evident, that if the laws of Nature are variable, those recurrences of perceptions in which habit consists, and on which all experience is built, could not take place ; nor could there be any foundation for those principles by which prudence and skill operate upon external objects, for the purposes either of common life or of science. The faculty therefore, by which animals are susceptible of that sort of habit which consists in the association of ideas, may be termed the organ whereby they perceive the uniform succession of cause and effect, established by the invariable course of nature. It was necessary that this should be an instinct, for the sake of self-preservation, not only to mere animals, but to the human species in infancy. If the noxious effects of fire, and the various modes of mechanical violence, such as falls and blows, were only to be learnt by a process of reasoning, all animals would perish before they could attain to maturity. The great difference of man and mere animals in this respect, seems to be, that the latter only perceive these associations, when the objects themselves are present to their senses, whereas the former, by being endowed with memory, can reflect upon them though absent, and thereby render them subservient to experience ; for, with regard to

external bodies, what is reason but experience; and what is experience, but the remembrance of objects as they affect each other, and the application of this knowledge to the practice of life, in adjusting means to ends, the constituent of what is meant by *Judgment* and *Art*? The principal difference of one man from another, in point of understanding, consists in the readiness with which the mind forms these combinations, and the strength with which it can guard against such as are accidental and fanciful, and discriminate these from such as have an archetype in the nature of things; and that mind, of which the conceptions correspond best with the real associations of nature, is possessed of correct judgment and just observation, the most valuable of all mental attainments. This faculty may be denominated Practical Induction, and all deviations from this sound state of the mind is morbid, and constitutes the various degrees and forms of misconduct, insanity, and delirium.

It would lead to disquisitions too long and too intricate, and, in some measure, foreign to this place, to enlarge further on the various effects of the combination* and succession† of ideas which connect animal with intellectual nature. I shall

* See Theory of the Moods of Verbs, by Dr. Gregory. Phil. Trans. Edin. 1790.

† See a Treatise on Time, by Dr. Watson, jun. F. R. S. Lond. 1785.

only remark, that those internal faculties upon which habit and association depend, carry a reference to external nature, exactly analogous to the mutual relation formerly mentioned as subsisting between *stimuli*, whether internal or external, and the moving powers corresponding to them, and between the organs of sense, and the impressions of external bodies which are naturally adapted to them. Muscular motion and sensation have relation to the single properties of matter, as they affect particular fibres and organs, but habit and association are co-relative to that stated connexion of cause and effect, established by the general laws of nature. We can thus trace a correspondence between the motions, sensations, and faculties of animals, on one hand, and the properties of matter on the other hand, from the lowest limits of animal and even vegetable nature, into the boundaries of intelligence.

The like accordance with the laws of nature is observable in the structure of animals, as in their motions and functions, as may be exemplified by that reference to the powers of gravitation manifested in the conformation of the limbs and the position of the viscera, as adapted to the natural motions and posture of the body.

It would appear, therefore, that there is a co-ordinance or *pre-established harmony*,* as it were,

* The learned reader need not be told that the author here refers to the peculiar doctrine of Leibnitz, and that he means

between the faculties of animals and the laws of external matter, which is the foundation of all the instinctive habits of animals, as well as the rational conduct of man ; and it is impossible sufficiently to admire that sublime contrivance by which the frame of animated beings is thus in all points adapted to the constitution of inanimate nature.

The other instinct that remains to be considered, is imitation. This is an action of which some brutes of no great sagacity are capable, and yet it is the foundation of some of the most important attainments of rational beings, particularly speech, which could not otherwise be acquired, and without which the powers of reason would be extremely limited.† Speech seems to be to thought what writing is to speech, or

merely to allude to it figuratively, without approving or adopting it. This celebrated philosopher held that the obedience of muscles to the will, is not to be ascribed to any physical connexion between the mind and body ; but that the Deity has so pre-ordained it that the actions of the mind and body should proceed by a parallel but independent series of movements, like two distinct machines, which without any mutual agency, are so constructed as to correspond simultaneously in their motions. Here is another theory of muscular motion, but as it is rather theological than philosophical, and being purely hypothetical and fanciful, *pace tanti viri*, it has not been thought worth while formally to advert to it in its proper place.

† See some ingenious observations on this subject in Dr. Campbell's *Philosophy of Rhetoric*, Book II. chap. vii.

rather what arithmetical or algebraical notation is to common language, whether spoken or written ; for without speech the operations of the mind, particularly that of abstraction, would be extremely limited, nor could there be any of those extensive combinations of thought which constitute a chain of reasoning. It would appear from this and from the remark in page 85, that all the operations of the human mind are founded on sensation, habit, memory, and speech.

In the early part of human life, imitation seems equally independent of reason and reflection, as in mere animals. It takes place not only without the operation of the will, but in opposition to it ; for yawning is an involuntary spasm of the muscles of the jaw, which is frequently excited by a sight of the same action in others ; and there is a case recorded in the *Philosophical Transactions*, by Dr. Garden,* of a man who, in his adult state, and possessed of reason, imitated involuntarily and irresistibly whatever gestures he saw in others. We are to account, on the same principle, for that general similarity of external manner, and of accent, as well as that conformity of principles and sentiments, observable in particular sects, societies, and nations, and which all men insensibly acquire in a greater or less degree.

The only objects of imitation, are gestures and sounds, and by these are also transferred from

* *Phil. Trans.* vol. xii. p. 842.

one individual to another, the emotions of the mind of which they are the natural expressions. The imitation of gestures seems, at first sight, less unaccountable than that of sounds; for it is performed by members which are objects of sight, and would therefore seem more easily transferable to the correspondent part of another person. Children imitate motions and gestures before they imitate sounds, and when they begin to articulate, they first attempt those letters, in the pronouncing of which the motions of the organs are the object of sight: these are *b*, *p*, and *m*, among the consonants, and the broad *a* among the vowels. But there are other letters and sounds, in forming which, the organs of voice are so hidden and minute, that we can have no knowledge of what parts are put in motion in order to produce sound. But upon further reflection, there seems little or no difference in this respect; for, independently of anatomy, we know nothing of muscles but by their effects; and there seems no reason why the *ear* being affected by a sound, should not excite a given motion in the muscles of the *larynx* and *fauces*, as well as that a gesture, by having its image impressed on the retina, should excite motions in the legs or arms. Even where imitation, or any other action, is the result of deliberate volition in rational beings, the motion is not performed from a knowledge of their having muscles. They only *will* the effect, without know-

ing by what means it is performed ; for though it may seem obvious that all the motions of an animal are effected by the shortening of the fleshy fibres, this is a fact with which those only are acquainted who have some knowledge of anatomy and physiology, and may be considered as a fundamental and first-rate discovery in the natural history of the living body. This discovery cannot be traced to any particular improver of physiology, but seems to have arisen, like many other discoveries in science and the arts, rather from the gradual evolution of knowledge than the efforts of any individual. It has at all times been observed, that exertions of strength produce a swelling and motion in the fleshy parts of the extremities, and the word denoting a muscle in different languages, is taken from the resemblance of the motions under the skin to those of a little nimble animal, such as a mouse or a lizard. A muscle is called in Greek *μυς*, in Latin *musculus*, or *lacertus*, and that muscle which is so visible near the ham of a quadruped when walking, is, in the common language of some parts of the country, called the *mouse*. It is not ascertained, so far as I know, who first ascribed the motions of animals to the contraction of fleshy fibres. There is no mention made of this in the works of Hippocrates, but it is very clearly stated by Galen ; so that the discovery seems to have been made in some intervening period. This property

of muscles is so well ascertained in modern times, that wherever we see a muscular substance, we infer synthetically that some corresponding function must belong to it ; and we find an irrefragable argument for the circulation of the blood, only from considering the heart as a muscular substance. This subject has not been well understood till modern times, otherwise the circulation would most probably have been discovered sooner ; and even since this discovery, we find some physiologists so little acquainted with the nature of muscular power, that they have invented a fanciful theory of the motion of the blood, by a supposed fermentation taking place in the cavity of the heart. We are chiefly indebted to Dr. Glisson, who lived about the middle of the last century, for the first correct ideas of the irritability and contractility of muscular fibres.

The last remark, or rather query, which I have to make on this subject is, whether mental *sympathies* may not be resolvable into imitation ? There are certain emotions of mind which are caught from one individual to another, whose feelings are as it were in unison, producing sometimes the most important, nay the most violent practical effects, in the history of human affairs. Of this, examples may be quoted in the paroxysms of fanatical zeal, bringing about crusades, and other vehement innovations, furious wars and tumults, civil and religious, of which

history furnishes abundant examples. It is a principle, however applicable to good as well as evil purposes, and one which poets, orators, painters and actors, know how to avail themselves of, as one of the main engines for giving effect to their art.

Ut ridentibus arrident, ita flentibus adflent
Humani vultus. Si vis me flere dolendum est
Primum ipsi tibi.

THERE still remains to be mentioned, that important property of living muscular fibres, which consists in a perpetual state of tension taking place at all times, in a greater or less degree, independent of any temporary stimulus. When any muscular fibre in a living animal body, whether in a fleshy muscle or a blood vessel, is divided by incision, there is an immediate retraction of the separated parts; and that this is their natural state, is further proved by the spontaneous motion which takes place in consequence of the relaxation of an antagonist muscle, as when the mouth is drawn to one side, in consequence of *hemiplegia*. A certain degree of this tension is necessary for the performance of the natural motions of the muscles, whether voluntary or involuntary, and the vigour with which the several actions are performed, depends on the fibres possessing a due degree of this constant tone giving

scope to motion. In order to maintain this tone, there must every where be a counteracting mechanical power; and we perceive accordingly, that the great muscles are kept on the stretch by the bones, the heart and vessels by the mass of fluids, and the intestines by the *ingesta*, and their natural contents.

The common integuments also have their salutary degree of tension. This is best exemplified in the scrotum, which being a peculiar duplication of the skin, its tone and laxity is more palpable than in any other portion of the surface of the body. Its loose and pendulous state therefore indicates a general languor, its corrugated and retracted state is expressive of vigour.

When this tension is either excessive or defective, various irregular and morbid actions are produced. The vascular system is more apt to be affected by various degrees of natural tone than any other part of the body; the reason of which may be, that this very relaxation produces a greater capacity of the vascular system, and the relative quantity of the mass of fluids being thereby diminished, the resiliency and energy of the vessels are not supported even by their former degree of distension. I have observed elsewhere,* that it is conceivable that those poisons which extinguish life in a moment, may act by inducing

* Elements of Medical Logick, page 93, where this subject is more fully treated.

an instantaneous loss of tone in the vascular system. An excess of it may arise either from the too great elasticity of the vessels themselves, with or without an increase of blood, or from simple plethora. The first is indicated by a hard pulse, and that corresponding state of the fluids which occasions in blood, when drawn from a vein and cold, a contraction of the crassamentum, and a sizzly crust. Simple plethora is most apt to arise in constitutions naturally too lax, and which, therefore, do not bear the loss of blood so well as the former.

A defect of tension in the vessels is produced either by disease, by hæmorrhage, or by natural constitution. In diseases, this want of tension is indicated by general debility and depression of spirits, and by a weakness of the pulse. And as irritability and sensibility are very much affected by tension, a want of it in the vessels chiefly constitutes what is called a nervous habit, such as is most commonly met with in the female sex; and there is nothing more apt to induce such a habit than hæmorrhage, which I have known to produce a long train of hysterical symptoms in those who had not formerly been subject to such complaints. The same principle is also well illustrated by the effect of a sudden removal of tension in the intestines. It is not uncommon for persons in a state of great debility to be affected with syncope, and even instantaneous death, in the

act of evacuating the bowels. It seems to be from a like cause that a temporary faintness is produced by the opening of an abscess.

There is a particular constitution incident to both sexes, which is commonly connected with corpulency, and has been called by authors the *temperamentum frigidum, phlegmaticum, and spongiosum*, and, in common language, a gross and flabby habit. In these there seems to be a deficiency of the natural elasticity of the vessels, and in certain diseases, even of the inflammatory kind, such as the erysipelas, to which they are liable, tonic remedies, such as the Peruvian bark, are found to be the cure, and even in some rare cases of disease of the lungs, chalybeate remedies have been found beneficially admissible. It is observable that corpulence is generally connected with flaccidity, the cause of which probably is that the adipose secretion stagnates in its follicles for want of that compression which a more tense state of the membranes, integuments, and other adjacent parts would give, and by which the absorption of the *adeps* would be promoted. The want of interstitial absorption in dropsy, is referable to a like cause, and to the want of that strong pulsation which belongs to a more vigorous arterial action.

There is, perhaps, no circumstance in which one individual differs more from another, than the natural tension of the muscular fibres; and

it would be more useful, as well as more conformable to nature, to found a discrimination of temperaments upon this, than on the fanciful theory of humours; for this difference of constitution not only gives occasion to a variety in natural aspect, but valuable inferences may be deduced from it in the pathology and treatment of diseases.

Not only the general excess or defect of tension, but the inequality of it, may be considered as a cause of disease. It seems highly probable that those local affections which depend on the congestion of fluids, are owing to the difference of tension in particular parts, in relation to the whole system. The whole arteries of the body may be considered as one vessel, the capacity of which is equal to the sum total of all the trunks and branches of the arterial system, and as every part must be equally distended by the mass of fluids, it follows that if the strength of the vessels of any one part should not be sufficient to support an equilibrium, they must yield more or less to the elastic pressure of the rest of the system, and produce disease. I believe this is one of the theories of local inflammation, particularly that of the erysipelatous kind.

There is, however, a circumstance of great importance in the animal œconomy, which must tend in some measure to counteract this inequality of tension. When the muscular fibres of

any particular part are under a state of more or less tension than the rest of the system, this is communicated by sympathy to every other part of the body. This is particularly observable in the blood vessels and intestines ; for a relaxation in any part of these will produce a like affection in every other part of the animal system. This principle of the animal œconomy has been better illustrated by Dr. Cullen than any other physiologist ; and he is of opinion, that great part of the effect of blood-letting in taking off the tension of the vascular system, in cases of inflammation, depends on the depletion of the vessels of the part from whence the blood is taken, for the proportion of the quantity drawn to the whole mass is very small ; and it may also be urged in favour of this opinion, that the more suddenly the evacuation is made, the more effectual is its operation in removing the inflammatory disposition, for the more quickly the local depletion is made, the less time is allowed for the restoration of the tone by the replacement of the lost fluid.

What has been hitherto said of muscular motion, has had relation to it as a property peculiar to animal matter and animal life. What I have further to add on this subject, will relate to the muscles merely as mechanical powers. As they constitute the strength of animals, it may be proper to consider the relation of their force to

their bulk, and the relation of the bulk and strength of the body to the density and cohesion of its own materials, and to the bulk, density, and cohesion of the external inanimate bodies with which it is conversant.

It has been demonstrated by Galileo,* that in similar unequal bodies of a cylindrical or prismatic shape, such as the limbs of animals nearly are, the ratio of their efforts to break by their own weight, is in the quadruplicate ratio of their lengths, but that the resistance they make to the same force is only in the triplicate ratio of their lengths. It follows from this, that in order to endow the limbs of animals with the same relative force, it is not only necessary that the bones should possess an increased proportion of thickness, in order to give an adequate increase of what may be called the dead strength, but a similar increase of living strength will be necessary, by a suitable addition of muscular power, in order to keep pace with the increased size of the bones. Now we observe, in fact, that in the large-sized animals, such as the bull and the elephant, the thickness both of their bones and muscles bears a greater proportion to the length of their limbs, than in the smaller animals, and they are therefore of a less elegant form. But Nature has not carried this so far, as to compensate for the

* Vid. Opere di Galileo. Discorsi e dimostrazione matematiche.

disadvantage arising from the increase of size ; for the greater animals have not the same proportional strength, in relation to their bulk, that the smaller animals have. It has been computed* that a flea can draw from seventy to eighty times its own weight, whereas a horse cannot with ease draw more than three times his own weight. This disproportion between strength and size is very observable in different individuals of the human species, when compared to each other ; for tall men are not muscular, even in the simple proportion of their stature. The difference in the shape and size of different men may be considered as an accidental variety, or *lusus naturæ*, owing, probably, to his artificial mode of life, and for which Nature has therefore made no special provision.

We are led, however, from a view of the same mechanical principles, to perceive the wisdom of Nature in assigning certain general limits to the stature of the human body. Had man been made much larger, he would have been unwieldy, and subject to accidents in his motions, in consequence of the *momentum* of the parts increasing in a higher ratio than their power of resistance. It may be answered, that the parts might have been made proportionally more hard and tenacious. But there are other circumstances in the animal œconomy which would have been a bar to

† Vid. Haller *Elementa Physiologiæ*. Cap. IX. Sect. II.

this ; for had the bones been harder, they would not have been calculated for the common duration of life, the effect of which being to increase their hardness and dryness, they must be endowed originally with a proportional degree of softness and succulence. And with regard to muscles, a degree of hardness, much greater than they naturally possess, would have been incompatible with their contractility. Another inconvenience of the greater stature of man would be, that he would require larger habitations, more food and clothing, while he would have less relative strength to provide for these wants. On the other hand, had man been of a stature much less than what he enjoys by nature, he would not have possessed sufficient power over external objects, to act up to those superior faculties of mind with which he is endowed, and which by his destination he is required to exert. If nature had conferred on man only one half of his actual stature and strength, with the same powers of reason, we may venture to affirm that he would not have carried his dominion over nature to the same extent. As he is now constituted, his force being commensurate with external nature, he has been able, either by force or artifice, to assert his sovereignty over the woods and fields, by mastering the strongest and fiercest wild beasts ; he has been able to change the whole face of nature on the surface of the earth, by works of industry, and

monuments of art ; he has been able to fell trees, to build ships, and to circumnavigate the planet he inhabits. It is rather a triumph of his reason than of his corporeal strength, to say, in the language of a modern poet,* that he can

“ Measure earth, weigh air, and state the tides ;”

or, according to the sublime idea of an ancient philosopher,† that he could turn the earth from its orbit, could he find footing on another earth, from whence to exert the powers of mechanism ; but such knowledge and such conceptions could never have been attained but by a being of a due degree of bodily strength and stature.

From what has been said, it may safely be inferred, that as the external bodies with which we are conversant possess given degrees of cohesion, bulk, and density, which require corresponding powers to act upon them ; so the human body, at its mean stature, is best adapted for producing those changes upon matter, which are necessary for self-preservation, and the various accommodations of life. And an argument may be drawn from hence against the tenets of those speculative philosophers, who hold that the size and strength of man were much greater in remote antiquity than in modern times. It is evident from what has been said, that if the bulk of the human body were much greater than it is,

* Pope.

† Archimedes.

it would be both useless and inconvenient, and would not preserve that harmony with the rest of nature, which is so agreeable to the analogy of her other works.

I SHALL conclude this Lecture with some remarks on the muscles, considered as mechanical powers acting upon levers.

The first remark to be made upon this, is so obvious, that it has hardly escaped the notice of any modern physiologist, and seems at first sight to militate against that wisdom of nature which is so conspicuous in other respects. What I mean is, the great waste of mechanical power which is incurred by the manner in which the muscles are inserted into the bones. This disadvantageous action of muscles is chiefly owing to two circumstances. One of these is their insertion, in almost every instance in which they are connected with bones, into a part which is much nearer the fulcrum than the resistance. Thus the two muscles of the arm, called the *biceps* and *brachiaëus internus*, in order to support in the hand a weight of one pound with the fore arm at right angles to the *humerus*, must exert a power equal to ten pounds. The other circumstance giving rise to a waste of power, is the great obliquity with which they are inserted into the bones upon which they are intended to act, so that the greater part of the force is expended in pressing one bone against

another at the articulation, and only a small portion of it in making the flexions and extensions ; so as to produce the desired effect at the extremity.

But these disadvantages are compensated by certain conveniences, and if nature has endowed the muscles with sufficient power for the purposes of life, after making allowance for the waste of force, there can be no reason to find fault with her management. One of the principal advantages arising from this distribution of the muscles, is the preservation of the shape of the members ; for unless the muscles and tendons had been pretty nearly in the direction of the bones, they must have passed like bow-strings from one bone to another, in making the flexures of the joints.

In estimating the waste of force, in consequence of the mechanical disadvantages before mentioned, we are to distinguish between those actions which consist in pressure, and those which consist in percussion ; for as the *momentum* of the latter depends on velocity, it is evident that there is a great advantage from the insertion of the tendon being near the centre of motion, as greater velocity, with less expense of contraction, will be thereby imparted to the extremity. The muscles, for instance, which are attached to the *olecranon*, in performing those actions with the hand which require rubbing, act with a disadvantage, exactly in proportion to the inequality of the distance

from their insertion to the joint of the elbow, and that from the same joint to the hand. This is an act of pressure. But in the case of percussion, as in the action of using a hammer, there is an evident advantage resulting from the velocity communicated to the extremity; for in order to have produced the same velocity, with the insertion at a greater distance from the centre of motion, a much greater range of contraction would have been necessary. The saving of contraction, therefore, may be reckoned another principal advantage in the attachment of muscles near to the centre of motion. As this is a point which I think has not been fully and closely investigated in explaining the mechanism of the muscles, I shall conclude with some remarks upon it.

As the muscles of voluntary motion are subject to fatigue, every circumstance that can tend to diminish this, will be favourable to the purposes of nature. Fatigue depends upon the force, frequency, duration, and extent of the contraction of muscular fibres. It is this last which is meant here to be illustrated. If any one will take the trouble of comparing the fatigue of the *biceps* muscle, in bearing a weight in the hand, with the elbow joint bent to a right angle, with that of bearing the same weight for the same length of time, with the joint at an acute angle, he will be sensible how much the degree of fatigue depends on the extent of contraction, and by attending to

the relative situation of muscular fibres, it will appear that Nature, in distributing the fibres of muscles obliquely, has had it in view not only to increase their number, but to save contraction.

In surveying the actions of all the various muscles, it appears, not only from the co-operation of different muscles, but from the position of the fibres in the same muscle, that there is hardly an action to be met with that can be called direct. In some instances, two muscles, or sets of muscles, are made to co-operate, so that the motion effected by them shall be in the diagonal of their direction. This is the case of the oblique muscles of the abdomen in some of their actions, and the intercostal muscles in all their actions. Sometimes, different portions of the same muscle produce in like manner an intermediate and combined effect, as in the instance of the *cucullaris*, one part of which being attached to the *vertebræ* of the neck, and another to those of the lower part of the back, their joint effect is to draw the *scapula* towards the spine. And in all the long muscles, however simple their origin and insertion may be, there is an internal obliquity of their fibres, in regard to each other, better described by the late Dr. Hunter in his Lectures, than by any former anatomist; for these do not run from end to end, but there are parts of the tendon running into the belly of the muscle, so as to divide it into penniform and rhomboidal

portions. This distribution of the fibres takes off from their length ; but as it takes place in those cases where the origin and insertion are at a considerable distance, this can be afforded ; and this, as well as the waste of power, in consequence of oblique action, is more than compensated by the increased strength, from the fibres being multiplied ; for, in consequence of this structure, there is an extent of tendon afforded sufficient for the insertion of a much greater number of fleshy fibres.

This principle in the mechanism of muscular action, is well illustrated by considering the motions of fish. The muscles of most fish consist of regular series of oblique and extremely short fibres, forming those flakes or layers which every one must have observed in their muscular substance. Their motions are more simple and limited than those of land animals, but much more vigorous ; for a fish in the sea has to make its way through a medium about eight hundred and fifty times more dense than air, and with more rapidity. Nature, therefore, instead of giving them muscles whose fibres would run straight from one end of their body to the other, has incredibly multiplied their numbers by the wonderful contrivance of distributing them into short and oblique portions. If one were called upon to name instances of the greatest muscular effects, it is in fish that these are to be found. I have

seen the sword of a sword-fish sticking in a plank, which it had penetrated from side to side;* and when it is considered that the animal was then moving through a medium near a thousand times more dense than that through which a bird cleaves its course at different heights of the atmosphere, and that this was performed in the same direction with the ship, what a conception do we form of this display of muscular power!

An advantage the reverse of what has been stated, arises from the oblique direction of the intercostal muscles, the fibres of which are thereby lengthened; for in parts so near each other as the ribs, there would have been a great inconvenience in their passing directly from one to another. Besides, in consequence of their oblique direction, the origin in the superior rib is placed nearer to the centre of motion than the insertion in the inferior rib, the effect of which is, that all the ribs are elevated, whereby the cavity of the thorax is enlarged, which is the view of nature.

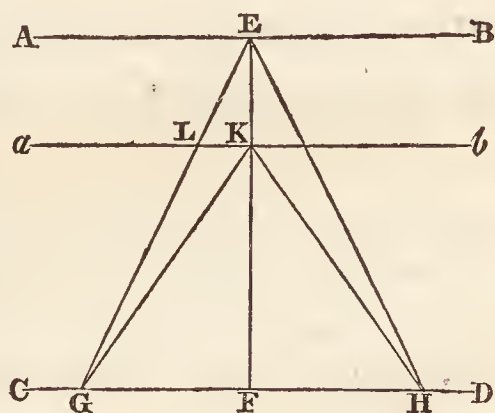
But the advantage or rather compensation of obliquity, which I mean particularly here to demonstrate, is, that the same effect is produced with a less proportional decurtation of fibres, than if the same motion had been performed by a direct power. Borelli has estimated geometri-

* There is a specimen of this in the British Museum where the weapon of this animal is seen driven through the wood, almost to its basis.

cally the loss of power from oblique action, but seems to have overlooked this compensation of it, which is not inconsiderable, when we reflect that there is thereby a saving of contraction, and consequently of fatigue. This can be rendered an object of geometrical proof, and I here subjoin a demonstration of it, which I made out, when engaged in the study of anatomy, in the year 1771.*

Let the line AB , in the annexed diagram, represent a moveable bone, and the line CD a fixed bone parallel to it. Let FE , perpendicular to these lines, represent a muscle acting in its own direction, and the lines GE , HE represent two muscles acting obliquely, and producing, by a diagonal action, the same effect as the other. If the bone AB be brought to the situation ab , by the action of the muscle FE , the muscle will then be in the situation FK . If the bone is brought into the same situation by the action of the muscles GE , HE , these muscles will then be in the situation GK , HK .

The proposition to be demonstrated is, that the line GK bears a greater proportion to the line GE , than the line FK does to line FE ; for FK is to FE as



* This principle was first adverted to by Dr. Mayhew, a

GL is to GE . (Euc. Elem. B. vi. Prop. 2.) and the angle ELK , being less than a right angle, the angle GLK , which is adjacent to it, must be greater than a right angle; and the angle GKL , being in the same triangle with GLK , must be less than a right angle. The line GK , therefore, which subtends the greater angle, is greater than the line GL , subtending the lesser, and therefore bears a greater proportion to GE . But the line GL is to GE , as FK is to FE ; and therefore GK bears a greater proportion to GE , than FK does to FE ; that is, the fibres of the muscles acting obliquely, suffer a less proportional decurtation than those of the muscle acting directly.

It is further obvious, that the more oblique the action becomes, the greater saving there will be of contraction; for in moving the line ab towards CD , the line FK diminishes in a swifter ratio than the line GK , and when the former has vanished, the latter is in the situation GF .

I have thus endeavoured to sketch some of the most important particulars in the natural motions of living animals, a subject which affords one of

physician of great genius of the seventeenth century, and has since been taught in different schools of anatomy, particularly by the late Dr. Monro, and such proofs of it were adduced in a popular way as were sufficiently convincing; but it has no where but here, that I know of, been demonstrated with geometrical rigour.

the finest and most fertile fields for contemplating the wisdom with which Nature adapts her means to her ends; and which has been justly considered as carrying the most irresistible evidence of the existence of an intelligent cause. The subject is so far from being exhausted, that I am convinced there are circumstances in the relative distribution and correspondence of organs, depending on muscular motion, so profound and exquisite, as far to exceed the utmost reach of human thought to comprehend, or of human ingenuity to detect; and here, as in every other part of the frame of the universe, the most elevated conceptions which the most enlightened understanding and most enraptured imagination can form of the beauty and magnificence of Nature, will fall far short of the real sublimity of her works.

DISSERTATION IX.

On the Yellow Fever.

THIS disease, which in its worst form has proved one of the most calamitous epidemics with which mankind has ever been visited, was unknown in the records of physic till the middle of the seventeenth century. It first made its appearance in the Carribean islands in 1647 ; and in fifty years afterwards on the American continent, at Boston, in New England. The first account of its appearance in Europe, was at Lisbon in 1723. Its chief havock has been in the sugar plantations of the West Indies, and my attention, as a matter of duty, was first called to it from my service in the navy having been chiefly on that station. It has there raged with little intermission for the last thirty years, with the desolating effect of a pestilence. In North America, its ravages have been equally deplorable, though not so general and unremitting during the same space of time. In particular spots in the south of Europe, chiefly in the sea-ports of the Mediterranean, it has shewn itself in all its horrors at intervals, from its appearance in Cadiz in 1800, till that at Barcelona in 1821. In the former

place it has appeared in all no less than ten times.

There is little wonder, therefore, that this disease should have excited a deep interest in our times in those states which have been affected by it. And though it has never visited the shores of Britain, the immense number of her most valuable subjects who have perished in the West Indies in the service of their country calls forth the keenest feelings of every heart not dead to every sentiment of humanity and patriotism.

This disorder, like all others of a pestilential and malignant nature, sets at defiance the art of physic in its curative capacity; human skill, of which the most anxious and judicious exertions have not been wanting, has been found of little avail in abating the fatality of this epidemic. The great, and only substantial source of hope to be looked to must consist in preventive measures. But from some peculiar circumstances in the nature and character of this disorder, there has most unfortunately been much difference of opinion, if it may be the subject of prevention, for great doubts have arisen in the minds of many whether it is contagious. The vast importance of deciding this question is too obvious to enlarge upon. From my own observations in the islands from the year 1780 to 1783, I had not much opportunity of seeing it in its worst forms. The

navy as well as the army, and civil population, were more than ordinarily exempt from it in these years, insomuch, that I saw more cases of what is popularly called the Yellow Fever, which were not of an infectious nature, than of those which were so. I saw enough, however, in the hospital at Barbadoes, and in the ships and hospital at Jamaica, to convince me of its contagious nature in certain circumstances; and from the best consideration I have since been able to give this subject, I remain persuaded that whenever it is so aggravated as to appear in an epidemic and pestilential form it is truly contagious. Under this persuasion, I must further be convinced that the neglect of preventive means cannot fail to be of the most fatal and deplorable consequence, and believing that some myriads of human beings have perished miserably from the contrary opinion, I have felt it my imperious duty to use my most strenuous endeavours to evince what I conceive to be the truth.*

The main sources of the ambiguity in this case have been, 1st, The want of discriminating the

* The greater part of this Dissertation will be found in the first edition of *Medical Logick*. Lond. 1819. The same subject is noticed, but slightly, in the second edition; for, I was then under the belief that the opinion of non-contagion had been nearly eradicated, but finding this not to be the case, I have deemed it my indispensable duty to repeat my utmost efforts in asserting the cause of truth and humanity.

remote causes ; 2dly, The similarity of some of the most prominent characters which are in common to all the forms of it ; 3rdly, The transition of the non-contagious into the contagious form in consequence of foul air and filth in hospitals and ships, or other crowded places, as happens commonly enough in the fevers of Europe ; 4thly, Its limited range in point of climate and season, the idea of contagion in some persons minds being that it is essential to its nature that it take place in all circumstances and situations. 5thly, Its not extending itself to the rural population, those only being susceptible who live in towns, or other dense and crowded communities.

All these will be duly adverted to in the course of this Dissertation.

In order to avoid the first source of fallacy, it is necessary to remind the reader, that there are three sorts of remote causes which give occasion to fevers, in whatever climates they may arise. One class of these causes is the exhalations of the soil, producing intermittent, and remittent fevers, which occasionally pass into continued fevers, particularly when under the influence of other remote causes.

The second class of occasional causes, is vitiated human *effluvia*, generated by the living body under circumstances of crowding, filth, want of ventilation, and change of apparel, aggravated occasionally by scanty and unwholesome food, as

exemplified in the jail, hospital, and ship fever, and that of the indigent part of the population : and all pestilential epidemics seem to have had a similar origin, diversified according to circumstances not always definable and ascertainable. In the pestilential distempers, recorded by historians and poets, which broke out in besieged towns and besieging armies, famine appears to have been a main element in their production. That described by Thucydides, at Athens, was quite different in its characteristic symptoms from the modern plague of the Levant. The pestilential disease called the Sweating Sickness, so fatal in England from 1486 to 1551, was still more different from it. It was engendered by the wretched state of the army brought to England by Henry the Seventh, previously to the battle of Bosworth. There are interspersed, in the records of physick, the histories of many other peculiar infections, some of them local, extremely limited, and transient. Infectious fevers, therefore, arise, prevail, and become extinct, under circumstances endlessly varied according to the indefinite combinations of the pre-disposition of the subjects acted upon by the different degrees and kinds of impure air, bodily and mental hardship, and privations. On this part of the subject also, the reader is referred to the *Dissertation on Infection* in volume i.

The third class consists of that disturbance of

the system, occasioned by fatigue, insolation, intemperance, the privation of food, and sleep, sudden alternations of heat, and cold, acting either jointly, or singly, in creating fever.

Of these three, the second only is found to be contagious.

They are all three found to exist in the West Indies, (by which is meant the Islands called the great and small Antilles, or Carribean and Mexican Archipelago, and the adjacent coast of America) in common with other countries.

But in conformity to what has been said regarding the irregularity of the *phenomena* of infection, there are found to be peculiar and unaccountable circumstances regarding it on this station, for the fevers originating from these three remote causes are all accompanied with a yellow colour of the skin, which being a conspicuous symptom, has procured for them all the appellation of yellow fever, giving occasion to great confusion, and serious mistakes. This colour of the skin is not quite unknown in fevers in other parts of the world, even in cool or temperate climates. I have met with it in London, both in my hospital and private practice, several instances of it have occurred in the typhus fever which lately prevailed in Edinburgh. Dr. Cleghorn met with a few examples of it in the endemic of Minorca; Sir James Macgregor mentions that a few cases of it occurred at St. Andero. It appeared in

a solitary ship in Gibraltar bay, in 1795.* But it is only in the West Indies that it is met with as a general and characteristic symptom of the endemic and epidemic fevers of the country. In this sense it is unknown any where else, even in other inter-tropical regions. There is no such symptom in the bad fevers of the East Indies. Dr. James Lind† has given one of the best accounts of that which prevailed at Calcutta in 1762, and this makes no part of its description. Dr. Johnson, in the history of an epidemic fever in Batavia, says, that the yellow colour of the skin occurred in the course of the disease, in a great number of cases, but this does not amount to a general diagnostic, as in the Antilles. It would be only matter of idle speculation and conjecture, to attempt to decide whether this peculiarity in the Antilles, is owing to some singular properties in the soil and air, or, what is more probable, to some casual concurrence of circumstances brought about by the peculiar state of these islands, such as has been remarked in the *Dissertation on Infection* at page 315 of volume i. It is conceivable that this peculiarity might have occurred from some assemblage of circumstances connected with the importation or treatment of

* This fact is taken from the *Journal of the Surgeon*.

† This is not Dr. James Lind, of Haslar Hospital, but a physician of equal learning and ingenuity, who passed the latter part of his life at Windsor, where he was most highly respected.

the African slaves. This is matter of conjecture, but it is matter of certainty that this fever cannot be traced further back than the middle of the seventeenth century, and that no such malady had existed till then in any age or any quarter of the world.

Another remarkable circumstance, with regard to the origin of the West India fevers, is, that they sometimes are found to arise from the foul vapour of ships replete with filth, from long neglect of cleanliness. And it is remarkable, that the fevers arising from this cause, are found sometimes to be contagious, and sometimes not, according to the intensity and nature of the effluvia, and the susceptibility of the subjects exposed to it. It seems to be a general rule, that no effluvia, emanating from corrupted dead matter, even in a state of the rankest putrefaction, ever produces a fever of a contagious nature. It is presumable, therefore, when these exhalations do produce contagious fevers, or convert a common fever into one of an infectious and malignant character, that they consist, in part at least, of the vitiated effluvia, generated by the living human body, constituting some form of the typhous morbidic poison.

In order to give that precision to language, which is necessary on every subject, and with a view to avoid misconception, and wrangling, these three classes of fevers shall be designated

as follows ; the first shall, in the course of this discussion, be called the ENDEMIC ; the second, the PESTILENTIAL,* or MALIGNANT EPIDEMIC, or TYPHUS ICTERODES, as it is very properly termed by some systematic writers ;† the third, shall be called the SPORADIC. A strict attention to this distinction will, it is hoped, go far towards clearing up the ambiguity and removing the dangerous errors with which this controversy is fraught.

All the three are in vague and vulgar language, styled the yellow fever, and the utmost confusion has arisen in treating of them, as must ever be the case, when one author, or disputant means one thing, and the other a different thing. It has accordingly been from want of precision, in naming and classing these fevers, that contests highly unbecoming a liberal profession, and what is infinitely more unfortunate, errors of the most fatal practical tendency have been engendered by this confusion, and ambiguity of terms.

This fallacy has in no instance been more glaring, than in some articles which have lately found admission into a respectable periodical

* By pestilential, it is here meant only to express a very high rate of mortality, and in this respect the epidemic in question takes the precedence of the plague ; for, on a population of 16,000 civil and military, at Gibraltar, the mortality in 1804 was 6000 ; a proportion considerably above the usual devastation of the pestilence of the Levant.

† Sauvages and Cullen.

publication.* The fever there described, is, by the author's own account, and avowal, evidently of the endemic kind, and in strenuously maintaining that this is not contagious, he is fighting a phantom of his own creation ; for no rational advocate of contagion, has ever alleged that a fever of this kind is contagious. The shallow and perverted reasoning of this and some other authors, would not have claimed notice, but as what they give to the world may prove mischievous, by some inexperienced or weak practitioner, applying what is advanced by them, to the pestilential epidemic fever, it becomes the peculiar duty of one, who has been forty years in the medical service of the State, to counteract the baneful impression it may make ; and as no example more apt could be selected to illustrate the necessity of verbal precision and discrimination in medical reasoning.

The fevers proceeding from long confined human effluvia, seldom originate in the West Indies ; for the heat of the atmosphere is such, that it is not necessary to exclude the fresh air, as in Europe, and other temperate climates. Such fevers, therefore, have become epidemic there, only in consequence of infection occasionally generated and imported by ships under peculiar circumstances of crowding, and filth, exasperated by

* See Medico-Chirurgical Transactions for 1818, vol. ix, part 1.

the length of the voyage, and various hardships affecting the minds and bodies of the crews, and passengers. Examples of these casual incidents are to be found in the history of the different maritime powers, who have planted colonies in this part of the world. Among the English, the most remarkable and well attested are that of a fever which arose in Barbadoes in 1647, as recorded by Captain Ligon, in which he says, that the mortality was so great, that the living could hardly bury the dead. He ascribes the origin of it to certain ships, which had just arrived from long voyages. The same epidemic broke out in the same island in the year 1695, as related in Hughes's History of Barbadoes; at which time, the former had been so far forgotten, that it was called *The New Distemper*; a proof at the same time that this, like the other, was totally different from the common endemic and sporadic fevers. But the most remarkable of all the epidemics of this kind, which affected the West Indies, was that which arose in 1793, first noticed in the Island of Grenada, in the month of March, a season, at which the endemic, and sporadic fevers, are the least prevalent; and favoured by the peculiar circumstance of the French revolutionary war which broke out that year, it spread rapidly to the British, French, and Danish colonies, attacking them not *simultaneously*, as it would have done had it been the endemic, but *successively*,

and therefore contagiously. It was also in every case traceable to intercourse with the infected. In the September of that year it reached Philadelphia, where it had been unknown since the year 1762. In the following year it visited Charlestown, in South Carolina, and in the subsequent year, 1795, New York, in all of which cities, it was attended with the calamities of a pestilence. It has since that period, at various intervals, visited all the maritime towns in the United States of America, from Georgia to New England.

I should have remarked, that this fever was believed on good grounds to have been brought to Grenada in a ship from the coast of Africa under peculiar circumstances of distress,* from

* The first and principal publication on this subject, is by Dr. Chisholme; who, like the inhabitants of Barbadoes in 1695, believed it to be a new disease, and calls it a *nova pestis*. This argued a want of knowledge of its history, but it argues at the same time that this belief could not proceed from *prejudice* or *prepossession*, but from the honest conviction of his mind, forced upon him by the irresistible evidence of the facts under his eye, to him new and singular. Dr. Chisholme seems also to betray a want of knowledge of the history of infection, in his great anxiety to prove that a malignant fever must previously have prevailed in this ship. It is well known, that typhous infection will accumulate to the most intense degree, without affecting those by whom it has been generated; and that it is on strangers that it exerts its virulent properties. All the medical practitioners of that island had the like convictions; only one disbelieved in it, and he but for a short time; and it is no small proof of its contagious nature, that six prac-

which it spread to the ships at anchor around her, destroying in a short time two-fifths of their crews. It then passed to the inhabitants on the adjoining part of the shore, spreading by steps, which could be clearly traced from the original infection in the ship.

In the year 1800, it made its way to the shores of Europe, and exhibited all its tragical effects at Cadiz, imported by a corvette named the *Dauphin*: also in a slight degree next year, but returned in 1804, with even greater violence than in 1800. Its next appearance here, was in 1813, and again in 1815. It broke out at Malaga in 1803, and in 1804 at Gibraltar, where it also made its appearance slightly, in the years 1810 and 1813, but was checked in its progress by vigilant measures of police.

It next spread, at various intervals, to Carthage and Alicant, and in 1804, to Leghorn, lat. 43,

tioners of medicine died of it, a mortality far greater than that of any other class of men. Dr. Stewart, a practitioner of nineteen years standing, and to whose excellent judgment, and long previous experience of the climate, the utmost deference is due, has on various occasions publicly attested his firm belief, that this epidemic was a disease quite distinct from any that had ever before come under his observation. Dr. Gilpin, physician to the army, bears the same personal testimony as Dr. Stewart. The latter informed me verbally, that he pronounced the disease to be distinctly different from the common endemic, before he heard of the ship from Africa, or any suspicion of its being imported in her, being then in the interior of the Island.

which was probably the most northerly place to which it could, by its nature, reach ; and it was here experienced in a very slight degree, thanks to the vigilance of Dr. Palloni, who opposed it as soon as the yellow skin and black vomit betrayed its nature. The most remarkable instance of its appearance in Europe of late years, has been at Barcelona, where it broke out soon after the arrival of some ships from the Havana, in June and July 1821. The yellow fever prevailed there at the departure of the ships, and more than twenty persons died of it on board of some of them, on the passage. The magistrates of Barcelona, at its first appearance, began to take measures of precaution for preventing its spreading, but being resisted by the inhabitants, abetted by certain physicians, who treated the opinion of contagion with ridicule, these measures were suspended. The consequence was, that in no place have its ravages been more horrible than in this city, and its environs. More than twenty thousand fell victims to it in the city alone, in the course of four months ; no part of the population being spared except those in prisons and the paupers in some charitable institutions, where rigid seclusion took place.* A committee of

* See Rapport sur l'origine, &c. de la fièvre jaune, translated from the Spanish, Paris, 1822. This committee consisted of members of the National Academy of Practical Medicine regularly appointed by the Government.—It is necessary to warn

Spanish physicians was appointed to consider and report on this subject. The great majority were in favour of the opinion of contagion. Four physicians were sent from France on the same service. One of them caught the fever, and died of it previous to the report, in which the three survivors were unanimous in favour of the existence of contagion. The conclusion of it, as drawn up by one of them, Dr. Pariset, is transcribed in the note.* This epidemic made its appearance

the reader against another report made by a self-constituted tribunal, at the head of which was the Author of an English work, the object of which was to disprove the contagion of plague. The facts so incontestably proved by the legitimate committee regarding the fever at the Havana, and on board of the ships on their passage, are here not only questioned, but flatly contradicted.

* “ Yes, the disease that now devastates Barcelona is truly the yellow fever of America.—Yes, it has been imported.—Yes, I repeat it a thousand times, it is contagious. Let it be hoped that the facts which we have accumulated will enable us to master the partisans of the contrary system.—Yes, this fever is a hundred times more pernicious to commerce than the most rigorous quarantine could possibly be. With only five days good police, and firmness, both Barcelona and its commerce might, humanly speaking, have been preserved, even on the avowal of the anti-contagionists of this country. But what has been the fact? They wrangled, they disputed; the scourge entered, raged, proved fatal, and nobody knew how to remedy it. Hence all labour, all industry, all prosperity is here extinct for a long time. The heart itself has partaken of this depravity. The father shuns the presence of his children, and there is an adieu to every feeling of humanity. Oh that the administration

at this time in several parts of the Mediterranean, but in none of them in which it could not be traced to the Havana infection. Among other places it broke out at Marseilles, but was promptly extinguished by efficient measures of police.

In tracing it further back, we find that its first introduction into Europe was at Lisbon in 1723, probably brought from Brazil, and it has never since appeared there. It next appeared at Cadiz in 1732, three years after its first appearance in the colonies. It returned in 1733, again in 1744 and 1746, and 1764, and not again till 1800; nor was it known in all that space of time, in any other part of Europe, except once at Malaga, in 1741. These dates are taken from Baron Humboldt, on whose accuracy and fidelity, perfect reliance may be placed.

To return to the West Indies: among the French, the most remarkable instance of this epidemic, is that recorded by Père Labat, of its introduction to Martinique in 1686, by a ship called the *Oriflamme*, under peculiarly calamitous circumstances, from Siam, whence it got the name of *Maladie de Siam*; and *fièvre de Matelôt*. And Père du Tertre, who wrote a history of the French colonies, in the end of the same century, calls it *La peste jusqu'alors inconnue dans les isles*. It was severely epidemic at Cap François in 1742.

had been unceasingly vigilant, and did not tamper in this amalgamation of follies and iniquities!"

In the Spanish West Indies, it is mentioned by the historians of that nation,* that it was unknown in those colonies till the year 1729, when it appeared at Carthagena, in Terra Firma; and that it broke out in Guayaquil, in Peru, in 1740. With regard to the Portuguese, it is distinctly described by Ferrayo de Rosa, a physician of Olinda, in Brazil, where it prevailed in 1687, immediately after the conquest of Pernambuco, and broke out on the arrival of the ship *Ori-flamme*, already mentioned, on her passage from Siam to Martinique.

In North America, the first mention of it is at Boston, in 1693, where it was believed to be brought by the fleet from the West Indies, under Admiral Wheeler; he came from Martinique, so that the infection was probably derived from what was imported in the fleet from Siam a few years before.

The next mention of it on that continent was at Philadelphia, in 1695, and at Charlestown the same year.

We next hear of it at New York, in 1702. There is no detailed history of it, but being designated by the appellation of the *Great Sickness*,

* See a Voyage to South America to measure a degree of the Meridian, by Don George Juan, and Don Antonio D'Ulloa. The same very respectable historians and philosophers say that those who have once had it are not liable to it a second time, vol. i. p. 46. English Translation, Lond. 1758.

and described as resembling the plague in fatality, it can be no other than our pestilential epidemic imported by commercial intercourse with the West Indies.

The first medical description of it is by Dr. Mitchell, of Virginia, in 1744, who imputes it to contagious effluvia.

The next professional account of it is by Dr. Lining, of Charlestown, in South Carolina, written in 1748, but comprising the description of this epidemic as it appeared there in the years 1732, 1739, 1745, and 1748. Till the first of these years, it had never been known in this colony. He says, it was undeniably infectious; and that in every one of these years, the introduction of it could be traced to persons arriving from the West Indies. (See *Physical and Literary Essays of Edinb. vol. II.*) There is no account of it that I know at Philadelphia in the last century, till 1751, when the infection was introduced in a trunk of clothes, belonging to a person who had died of it in Barbadoes.* It spread only to the family and a few of the neighbours, and no more than two hundred persons died of it. But it was not pestilentially epidemic till 1762, and did not re-appear there till 1793, as before mentioned.

There are two questions at issue with regard to this epidemic. 1st. Whether it has been occa-

* See the works of Dr. James Lind, of Haslar Hospital.

sionally engendered on board of ships, and carried to the sea-port towns of the West Indies, and from thence spread, by contagion, to North America, and Europe: or, whether it is only a different degree and form of the endemic, and sporadic fevers, which at all times prevail more or less in these countries? The other question is, whether it is infectious? In the agitation of this controversy, these questions have in general been regarded as involved in one issue; importation being considered as implying contagion. But on this subject, a schism has arisen among the non-contagionists; for Dr. Bancroft had allowed, in his *Essay on the Yellow Fever*, 1811, and still more explicitly in the sequel to that work, 1817, that this disease might be excited by exhalations from the ballast, and stores of ships, while he denied that it could be conveyed on men's persons, or that when excited by these exhalations, it could become contagious. He adduces, in proof of this, the fact, that the fever in the cases in question, near New York, did not extend beyond those who had connection with the ships, overlooking however a passage in the narrative, stating, on an authority which he will not question, that of Dr. Edward Miller, the great champion of non-contagion, that the whole inhabitants fled from the spot, "by which (he says not very consistently) it was suddenly arrested." It may here be remarked, that the unsophisticated good

sense of the inhabitants of the countries in which this epidemic has prevailed, taught by direful experience, every where fly from it with instinctive horror.

Dr. Miller, and others of the party of non-contagionists, will not however concede to Dr. Bancroft, that the fever can originate in any form, from a foreign source, continuing sturdily to maintain its exclusive domestic origin. To this there are two exceptions, Drs. Lidyard, and Lord,* both of whom publicly renounced their long entertained opinion of its endemic nature, and yielded to the irresistible evidence of its foreign origin. We have not learned whether they became converts to contagion, as well as to foreign origin, but the first mentioned of these candid and ingenuous gentlemen, has since unfortunately fallen a victim to the disorder.

But this is not the only point, in which importation, and non-contagion, have been disjoined, for Baron Humboldt, while he throws great doubts on importation, founding his opinion on the writings of Rush, and others to whom he refers, expresses his firm belief in the contagious nature of the Andalusian epidemic, founding it on the very able report of the three commissioners appointed by the French Government to investigate it, and who describe it as spreading from

* See American Med. Phil. Regr. vol. i. page 484, and vol. i. page 299.

house to house, by contiguity, as a fire does ; and he states it as fully ascertained, that those families who shut themselves up in the midst of it in Cadiz, escaped it.

It has been objected by the other party, that those commissioners were not on the spot when the epidemic prevailed. If this objection were well founded, it would go to invalidate all judicial investigations whatever. It is not deemed a necessary qualification for a judge on the bench that he should have been actually present at the transactions upon which he is to decide. On the contrary ; by an accurate and comprehensive survey of the points and bearings of a complex case, he is better qualified to form an opinion, than the actual actors in them, besides being divested of prejudice. It is on this principle that a court of justice is the only fit place for investigating matters of fact. It is requisite, for the forming of a clear, calm, and impartial judgment, that objects, whether natural or moral, should be placed at a certain distance, in order that they may be seen comprehensively in all their relative positions and bearings, which the eye and mind of a close observer, or of a party concerned, is incapable of taking in and duly appreciating.

There was the same proof of the existence of contagion at Gibraltar in 1804, as at Cadiz, for the Spanish Consul, Colonel Fyers of the Engineers, and others, disregarding medical opinion,

saved themselves and their families by seclusion.

In considering this subject on the general principles of abstract reason, it seems much more presumable, that the malady in question should originate from a fortuitous cause, than from the common course of nature. Is it conceivable, that a disease, totally different from any till then known, in the memory of man, by tradition, or history, should, in the course of seventy years, as at Cadiz, make its appearance six times, at unequal intervals, and in no other spot in Europe, except once at Malaga, unless from a foreign cause? and it is remarkable, that the degree of prevalence in those parts of the old world was in exact proportion to the degree of intercourse with the new world, and with each other; for the ships from the Spanish colonies hardly frequent any European port but that of Cadiz;* and the intercourse of this city with Malaga, Carthagená, Alicante and Leghorn,† and

* Since this was first written, it has appeared here several times in a mitigated degree, so as to make the number of its visitations to this city to amount to ten. Till the late revolutionary irregularities, all ships from Spanish America, except a few belonging to the Caraccas Company, which were permitted to go to a port in the bay of Biscay, were compelled to use Cadiz as their port. Is it possible that any one can believe that such a great comparative frequency of it at this spot had no connexion with its American intercourse?

† A most accurate and satisfactory account of this endemic

of these with each other, is more frequent, and constant, than with any others. Nor has this epidemic ever made its appearance either in rural districts, or in any inland or uncommercial towns, such as Rome, Naples, Palermo, and others lying in the susceptible latitudes. Nor has it appeared in Turkey or Africa, though in the same latitudes, except in one instance in the latter, in that part which is adjacent to Spain. Again, is it conceivable, that during the hundred years, that Gibraltar had been in possession of the English, that is from the year 1704, when this fortress was taken by the army under the command of the Prince of Hesse, to the year 1804, in which this pestilential epidemic fever for the first time broke out, this disease should never once have shewed itself, if it depended on causes at all times existing, and present? When it is gravely affirmed by a medical authority, that this singular, and till then unheard of epidemic, could here proceed only from the exhalations of the soil, and when the circumstances of this arid rock are taken into account, the author appeals to his reader, whether a proposition more extravagant, more repugnant to reason, more irreconcilable to history, and analogy, ever fell from the mouth or pen of any man? Is it not inconsistent with every conception of an endemic disorder, has been given by the learned and judicious Palloni of Leghorn, demonstrative of its imported and contagious nature.

and contrary to observation, and experience, that it should thus be unknown for so long a series of years? and considering this abstractedly, is it not repugnant to the first principles of reason, that a casual effect should proceed from a constant cause? The same may be said of Cadiz, the whole surface of which is either rock or sand; and while these pestilential epidemics raged in Cadiz and Gibraltar, the districts around, which are really marshy, were entirely free from it.

The like reasoning will apply to the occurrence of this fever in the West Indies, and North America; and when it is further taken into account, as an additional element of computation in the doctrine of chances, that these new and singular events, thus combined in each of these tracts of the globe, widely disjoined indeed, but in a state of constant intercourse, fell out *successively* in all of them in the course of one and the same series of years; it is quite inconceivable on principles of calculation, as well as by the laws of nature, that these events should happen by fortuitous co-incidence, and without the least relation to each other, as cause and effect.

Under an abstract view of the question, it is also presumable, and nearly demonstrable *a priori*, that unless the yellow fever, vulgarly and loosely so called, had been in some circumstances contagious, in others not, so much ambiguity and diversity of opinion could not have arisen. No

such controversy has arisen regarding the intermittent fevers of the fens of Lincolnshire, and the marshes of Zealand, which are universally admitted to be endemic, and non-contagious; nor do they vary greatly at any intervals of time; but that they should take on a pestilential form at the season of the year least liable to them, as was the case with the malignant fever of Grenada, would be out of all bounds of probability. And if the fever in question were in all circumstances equally non-contagious, could any doubts of its contagious nature ever have arisen?

Having discussed the presumptive proofs in favour of the foreign origin, and contagious nature of this epidemic, on abstract principles, let us take a review of the matters of fact which can be adduced on the same side of the question.

1st, It has never shewn itself in the first instance but in a sea-port town, and never in the interior of the country, whether island or continent.

2dly, It has, in most cases, been ascertained, that it has made its appearance in that sea-port, after the arrival of one or more ships,* either under those peculiar circumstances which engender infection, or conveying the infection from ports where it had already existed. Most of the great epidemics of the West Indies, North America, or Europe, can be traced to one or other of these

* See this exemplified at Barbadoes and Grenada, page 122; at Martinique, page 127; at Brazil, page 128.

sources. If it cannot in every instance be traced, this may happen from the want of historical facts : neither does it follow that infection does not exist, though it cannot be traced, for nothing is so subtle as infectious *effluvia*. It is well known, that small-pox and measles find their way where there is no possibility of tracing the source of the infectious matter ; but will any one deny the existence of small-pox, or measles, in a family, because the source of them cannot be traced ?

3rdly, No part of the population of the towns where it has broken out, has been affected, but such as had communication with shipping, directly or indirectly. A striking example of this has happened at New York since the first publication of this Dissertation. In consequence of a negligent administration of the quarantine in September 1819, some sailors belonging to ships from Baltimore, New Orleans, and the Havana, at all which places the malignant fever then prevailed, were permitted to land at one particular wharf. The fever broke out at that spot and spread to the immediate vicinity ; but having been arrested by vigilant measures of police, the rest of the city was saved from the impending calamity.*

The authorities for this opinion, besides the strong ones already quoted, of Dr. Mitchell and

* This is on the authority of a letter from Professor Hosack, of New York.

Dr. Lining, are the testimonies and writings of Sir James Macgregor, Sir James Fellowes, Mr. Pym, Sir Joseph Gilpin, Dr. Stewart of Grenada, Dr. Gordon of St. Croix, Dr. Arejula of Madrid, the French commissioners already mentioned, also those sent to Barcelona in 1821, and many other equally candid, competent, and honourable men, who had the best opportunities of closely investigating the subject.

The doubts have been maintained more plausibly in the West Indies than in the temperate climates, on account of the resemblance of this epidemic to the endemic, and sporadic fevers of those colonies; but the testimonies of Mr. Pym,* Sir Joseph Gilpin, Don George Juan, and Don Antonio d'Ulloa, and others, go directly to the point, and cannot be invalidated without impeaching the moral character of these honourable men; a species of argument, however, which it is to be deplored, has not in every instance been abstained from by the partisans of this question.

With regard to North America, evidence the most overpowering on this subject is to be met

* See a very concise, plain, and satisfactory account of this matter, in an article in the 5th volume of the Med. Chirurgical Transactions, by Sir Joseph Gilpin, who had the advantage of seeing this epidemic in Grenada and Martinique, as well as Gibraltar. But the work of Mr. Pym is the most full and convincing; and the most insuperable objections to non-contagion will also be found in that of Sir J. Fellowes.

with in the American Medical and Philosophical Register, in four volumes, published at New York, in the year 1814. There are in this work such a multitude of clearly ascertained facts in proof of its foreign origin, and contagious nature, that there is not room here for the bare recital of them.

My own humble efforts have not been wanting in the same cause. In the year 1798, I wrote a letter to Mr. Rufus King, Minister from the States of America to the British Court; and in the year 1805, another to Baron Jacobi, Minister from Prussia, for the information of their respective governments. In these letters, I laid particular stress on what occurred regarding a French ship taken in battle on the coast of America, in May 1795, on board of which this fever, or its infection, was found, and was communicated to the seamen of the British ship Hussar, by the men in health, who were shifted into her from the prize. It is evident that if it could be proved that this fever is communicable from one ship to another at sea, such a proof of the reality of contagion would be of the nature of an *experimentum crucis*, there being no possibility of land exhalations to account for it. Such I then considered, and still consider the facts of this case to be. They were, however, so strongly and speciously contested by Dr. Bancroft, as greatly to frustrate the impressive effect which my statement was calculated to

produce. The reader will be able to judge of the solidity of his objections, from an annotation at the end of this work. I feel that I was so far from making too much advantage of these facts, that I might and ought to have availed myself of them still more. I might have adduced them as a very striking illustration of the incompatibility of this disease with a certain temperate degree of atmospheric heat; for the change into cool and pure air, in proceeding to Halifax, did, in a very short time, first deprive it of its malignity, and then of its infectious nature, so as entirely to extinguish it. The few that were seized, after arriving at Halifax, might have imbibed the poison in the warmer latitudes through which they passed. It was on the strength of such facts as these, that, in my conferences with the members of the British Government, and in my correspondence with those of Russia and Prussia, I ventured to assure them, that in none of those countries was there any thing to fear from the importation of this pestilential epidemic, which in the end of last century, and the beginning of this, had so afflicted the West Indies, North America, and Spain, as to excite a general alarm throughout Europe. My anxiety to establish this, and the very extraordinary means resorted to by the advocates of non-contagion to invalidate it, will be readily understood, when the great and decisive importance of it is duly weighed. For if

there is any such thing as a law of evidence, or if there is any infallible criterion of truth to be found, it is undeniable and incontestible, that if it can be proved, even in a single instance, that this disease has been communicated from one ship to another *at sea*, the controversy is at an end. Let those, then, who maintain the opposite opinion, either come forward and invalidate this fact, or give up the point, on the issue of which the decision of the question depends. I challenge any one to point out the smallest ambiguity in it; and if there were any shadow of doubt, there are other parallel cases equally cogent and well attested to furnish a redundancy of evidence; for there have occurred, since the period alluded to, other cases besides this of the Hussar, equally conclusive, regarding the communication of this disease, from one ship to another at sea. It will be enough to specify two, in one of which the proof is even stronger. A French ship of war, the *Palinurus*, lying at Martinique, severely affected with the yellow fever, was ordered on a cruise to try the effect of sea air on the disorder. She fell in with and captured the *Carnation*, a British sloop of war on her passage from England, part of the crew of which were seized with the fever while at sea.* Another French ship of war, in which this fever prevailed both at St. Domingo, and on the passage to Brest, made prize of a mer-

* See Dictionnaire des Sciences Médicales.

chant ship from the Mediterranean, off Cape Finisterre, and having, without shifting the prisoners, sent a party of their own seamen to navigate her, the crew of the prize caught the fever, and almost all died of it.* The men belonging to the prize having been seized on board of their own ship, makes this a stronger case than those in which the prisoners were carried on board of the capturing ships, for in the latter case it might have been said, that the infection was derived from the exhalations of the hold or stores, whereas, in the former, it could only be from personal contagion.

There is still another useful remark, which I did wrong in omitting in my statement. Of fourteen men sent from the Hussar to navigate the prize, nine died before reaching Halifax, a passage of twelve days; the other five were sent to the hospital, where some of them probably died. Now, though it is mortifying to reflect that medical means should not have more control over this disease, this statement seems to afford the consolation of reflecting, that these means are better than none at all; for an opportunity here offered, which but rarely occurs, of ascertaining what are the results of the spontaneous tendency of unassisted nature. There was a mortality of nine in fourteen, and those who were sent on

* See *Traité de la Fièvre Jaune*, par Louis Caillot, Dr. en Médecine, Paris, 1815, p. 202.

shore, not having had the benefit of medical attendance, at that stage of the disorder at which remedies are most availing, did probably not all survive. This is a rate of mortality far exceeding that of the most unsuccessful practice, even that of Hippocrates.

4thly. The effect of quarantine regulations and vigilant police in shutting it out, and repelling its first assault, and the equally effectual and salutary result, of shutting up in the midst of infection. All these good effects have been experienced at Gibraltar, and elsewhere. It has already been mentioned how it was arrested there, *in limine*, in the years 1810 and 1813, and that many families were preserved in the midst of the desolating epidemic of 1804, by cutting off all communication with the garrison and inhabitants. The effect of seclusion at Cadiz, has already been mentioned. The persons in the jail, hospital, and poor-houses of Philadelphia, remained exempt from the pestilential epidemic in its utmost rage, all external intercourse having been prohibited. The same was observed of prisoners of war at Jamaica; and last year, 1821, the same is attested to have happened at Barcelona.* The American Register abounds with innumerable and irrefragable proofs of the good effects of seclusion and quarantine; and they ascribe to the more vigilant execution of the regulations of the latter, the ex-

* See Rapport sur l'Origine, &c.

emption from it at New York since 1805, at which time, as well as in 1803, they make no doubt, that it had insinuated itself in consequence of the loose measures of the quarantine, which, by a singular and unaccountable infatuation of the American Government, had been put under the directions of professional persons, who avowed their disbelief in importation and contagion. It may also be remarked here, that during the whole of the American war, from 1775 to 1782 inclusive, no epidemic occurred, an immunity which they ascribe to the intercourse with the West Indies having been suspended.

It has been admitted by one party of the non-contagionists, as already mentioned, that this fever may be imported by ships having foul ballast, or tainted stores. Dr. Bancroft, one of those who admits this, has been at great pains in another part of his work, to shew that no accumulation of filth, however great, and however putrid and corrupt, can produce febrile diseases on shore. It would have been satisfactory, if he had specified in what peculiarity the exception of the holds of ships was founded. I have the good fortune to agree with him, both in thinking that febrile miasmata do not in any case consist in the exhalations of simple putrefaction, and that fever may be produced by the exhalations from the holds of ships. But I am at no loss in specifying in what the corrupted exhalation of the latter, differ from

the former, namely, in their involving morbid secretions, particularly the *sordes* of the skin, and tainted *effluvia* of the living human body. These may long lie latent, and harmless, both to the crew and passengers, who become habituated to them on long voyages; but immediately affect visitors, on the arrival in port, particularly when the foul materials come to be stirred, as I saw strikingly exemplified in the French prizes at Jamaica. (See Diseases of Seamen, 3d. edit. page 88 et seq.) It forms no objection, therefore, to the importation of a fever being referred to a particular ship, that the fever did not actually exist on board of her at her arrival.

It becomes a curious and interesting, though painful question, what are the grounds upon which this deplorable and mischievous delusion is founded, for every error must originate and rest upon some false and mistaken principles, and these must have been of a nature uncommonly plausible and seductive, to have won so many partisans of respectable talents, and unquestionable good intentions. Of these the following seem to be the chief.

1st, The great similarity of this pestilential epidemic, to the endemic, and sporadic fevers of the Antilles. The most conspicuous point of resemblance, is the yellow colour of the skin. The resemblance, however, is not perfect here; for

the colour of the former is a dingy orange, in the other two a bright yellow.*

There is another symptom, in which all the three species bear a resemblance to each other; the vomiting of a coffee-coloured liquid in the dangerous, and almost hopeless stage. This symptom, however, is by far more frequent in the pestilential epidemic than in the other two. It was so striking and constant in the former, that the Spaniards gave it the name of *Vomito Prieto*, dark coloured vomit, when it made its first appearance among them, which was at Carthagena in 1729, as already mentioned, and it has retained that name ever since, though sometimes called *Fiebre Amarilla*. The College of Physicians of Philadelphia, besides mentioning the dusky colour as distinguishing it from the endemic, states, that it differs from it also in having no intermissions

* I have elsewhere, (See Diseases of Seamen, page 411.) started a doubt, whether the yellow colour was owing to bile, but rather to some *error loci*, or depraved state of the red globules. 1st, This colour does not appear first in the eyes, as in jaundice. 2nd, Sir Isaac Newton observes, that the blood reduced to thin *laminæ*, assumes a yellow colour. (See Optics, B. i. P. 2. Prop. 10.) 3rd, The like colour appears in *ecchymosis*, some time after a contusion. The yellowness of the yellow fever sometimes does not come on till after death. It seems deducible from these facts, that this colour may be owing to the red globules getting into the colourless order of vessels, in an attenuated, or vitiated state, and not to absorbed and circulating bile.

in the first days. According to the description of Dr. Chisholme, and others, the epidemic is distinguished also by violent pains in the head and legs, a piercing pain in the eyes, generally in one eye, more aggravated sensorial affection, such as coma and delirium, and above all by a greater malignity, that is, a much higher rate of mortality and resistance to remedies than in the other two species. The characters of this fever, as distinguished from the ordinary fever of the climate, are depicted in lively colours by the surgeon of the *Eurus* frigate, in his journal, examined officially by me in 1797. In a few days after the arrival of this ship at Grenada, eighty men were rendered unfit for duty, that is, about a third part of the crew: the eyes swam, as it were, in blood, with excruciating pain chiefly in one eye: the eye-ball started in one: in seven cases, one of the eye-balls was absorbed: in the course of convalescence, some lost their sight, though they retained the substance of their eye. Who will say that this differs in nothing from the endemic and sporadic fever of the Carribean Islands? though it may in many other cases have been so moderate as not to be distinguishable from that of sporadic or endemic origin; just as many cases of the true plague were not distinguishable from continued fevers. One of its further and most material distinctions is, that few of those who have had the true typhus icterodes, or pestilential epidemic,

are liable to it a second time. This must be understood under the qualification applicable to all diseases of this kind. Small-pox and measles admit of the fewest exceptions, scarlet fever more ;* vaccination as a security against small-pox, still more. The principal Spanish author, Dr. Arejula,† adduces incontrovertible facts in proof of those being safe from a second attack who have once had it. The same fact is equally well attested by Sir Joseph Gilpin, Sir James Fellowes, and Mr. Pym, under their own eyes, so that there does not seem to be the possibility of a fallacy. It is established by all the laws of evidence : by the multitude of cases, and the concurring testimony of persons who had no concert with each other, and no motive but that of truth. It is evident that no such inference could be drawn from a few cases, but the induction is founded on the cases of thousands, and the fact was so well established, that the Spanish Government made a proclamation, requiring those who had passed through the fever in the former epidemic, not to quit Cadiz, but to lend their assistance to the sick ; and both Spanish and English selected

* Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, vol. iii. page 445. London, 1800.

† See his work entitled *Brieve Descripcion de la Fièvre amarilla*, Madrid, 1806, page 229. This is a work of extraordinary merit, bearing the characters of great industry, accuracy, and sound reasoning.

their nurses from among those who had had it. Time and experience had so firmly established this confidence, that when this epidemic shewed itself here in autumn 1819, those who had previously passed through it, were under no fear or alarm, and were not anxious either to quit the city or to have recourse to seclusion with a view to avoid it.

Those who deny contagion, adduce strong facts in favour of the opposite side; but they are evidently taken from the endemic cases, or are mere exceptions. And there cannot be a stronger proof than this, of the reality of the difference of the two disorders, so that these facts may be said to militate in favour of contagion.

I have not experience of my own, to decide on the various points of difference, for the four campaigns in which I served in the West Indies were in years comprehended in one of those intervals before alluded to, between the appearance of these great epidemics. The mortality was indeed comparatively very moderate there, during the whole of that war, chiefly owing, no doubt, to there not having been great bodies of land forces transported thither, during that time, the war having been almost entirely a maritime one, and from no ship specifically infected, having arrived at any of the ports on the station. Some mixture of it with the endemic and sporadic, did occur both at Barbadoes and Jamaica; but, from want

of predisposed subjects, it did not spread. There were clear cases of it at the hospital of Barbadoes, evidently produced by overcrowding, as so frequently happens in the typhus of Europe, and the much greater mortality of medical officers at Jamaica in 1782, gave strong presumption of its existence there. My recollection of the above mentioned occurrence at Barbadoes is the more fresh, from the remarkable circumstance of a young negress employed as a nurse, having been seized with the most unequivocal symptoms of this fever, though it had been affirmed that neither females nor negroes of either sex were liable to it. There are other proofs equally satisfactory of the endemic fever degenerating into the contagious, in an article of great merit by Mr. M'Cabe, Surgeon of the York Rangers in Trinidad, in the year 1817, inserted in the Edinburgh Medical Journal. In a letter also from Mr. Laing, an army surgeon in the same island, inserted in the 2d vol. of Dr. Trotter's *Medicina Nautica*, the transition of the endemic yellow fever to the malignant is clearly evinced.

But admitting the symptoms to be ever so similar, it does not follow that they are *identical*. A great proportion of the cases of the true plague, were without the *tokens* or diagnostic characters, and some could not be distinguished from a continued fever, as already remarked; but in spite of this close resemblance, plague and fevers are

essentially different in their nature. Great inconvenience arose here also from their being externally undistinguishable ; for we find that in the history of plagues, both in England and France, particularly at Marseilles, there were sharp professional contests between the contagionists and non-contagionists, the latter, pertinaciously denying the existence of it, and sometimes so far influencing the public authorities, that precautions were so long deferred as to allow the malady to get beyond the reach of prophylactic means. Will it be said, because there are ophthalmias resembling the infectious ophthalmia, there is therefore no such thing as infectious ophthalmia? Has it not also occurred to every practitioner in this country, to see sporadic fevers, when there was not the least reason to suspect contagion, so far resemble the typhous fever from infection, as to be undistinguishable? Those cases of erysipelas,* which arise from infection, do not differ in appearance from those which arise spontaneously, but are very different both in their nature and treatment. If it can be proved that the malignant epidemic has a different origin, and greater fatality, to be of a contagious nature, and never, or very rarely, capable of being caught a second time ; these are sufficient diagnostics, let the outward symptoms be ever so similar.

* See Transactions of a Society for the improvement of Medical and Chirurgical Knowledge, vol. xxii. page 213.

2dly, The doubts, respecting the conveyance of this infection from the West Indies to North America and Europe, and the consequent doubt of its identity, may have proceeded from the fever in these temperate climates, being more extensive and fatal in its ravages, than in the country from which it is alleged to have been derived. Though it was severely afflicting in the Antilles, it did not attack so great a proportion of the population by far, as in Philadelphia, New York, Cadiz and Gibraltar. A very little reflection will clear up this difficulty. In the West Indies, the susceptible subjects are chiefly the new-come white people from Europe, for a very small proportion of the seasoned white inhabitants, and a still smaller of the creoles and negroes, are affected by it. But in North America and Spain, the whole population is in the same predicament; with regard to predisposition, as the white newcomers in the West Indies, who constitute a very trifling proportion of the whole population.

3dly, They allege, that an infection which vanishes of itself on the approach of winter, in North America, and even of the mild winter of Andalusia, and which does not spread among the adjoining rural population, is either no infection at all, or does not deserve that name; that the name of infection can only be applicable to such a disease as the small-pox, which makes no distinction of climates and seasons. But in the first

place, we can see no reason for believing that all infections are governed by the same laws.* Variety, is as characteristic a feature of nature as uniformity, particularly in all that relates to organic beings and animal life. Mere analogy can only be held as presumptive evidence, and may serve as a fair ground for rational conjecture and suggestion, but must ever stand subordinate to facts and observation, and may widely mislead us if too much confided in. It is little better than gratuitous assumption therefore, to say, that every infection must conform itself to that of small-pox, and the history of other infections militates against such an assumption. Would it not therefore, be more conformable to reason and sound philosophy, to reconsider, and recast theories, in order to make them quadrate with facts, than to strain, suppress, and deny facts, and question the veracity of honourable men, because they cannot be brought to conform to the theory? But if analogy were to decide the question, a strong and obvious analogy might be alleged in

* See a judicious arrangement of the varieties of contagion by Professor Hosack of New York, in the American Medical and Philosophical Register, vol. iii. page 14. Also an article in the Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, vol. iii. page 441.—The Author has endeavoured in Dissertation VI. in this work, to bring together all the known distinctive and comparative characters of various infections, on which to found a classification of them.

favor of this infection being controlled by circumstances of time and place, taken from the history of the true plague. This latter epidemic has never been known either in the tropical or arctic regions, from what cause it is not known. I profess myself unable even to guess whether difference of temperature acts upon the poison, by exhaling or decomposing it, or on the living powers of the body, by rendering it unsusceptible of its action. It would be still more difficult to assign a cause for one range of heat being necessary for the existence of plague, and another for the existence of the typhous icterodes. These are matters of pure observation, and so far from being founded upon or suggested by theory, as some anti-contagionists have affected to allege, they baffle all theory; and I can say for myself, that so far from having any theory, I am incapable of forming any hypothesis or conjecture on the subject.

But the knowledge of this is by no means necessary. It is the fact only, which is wanted for the present purpose. And it is incontrovertibly established, by the experience of ages, that the existence of plague cannot co-exist with a heat of the atmosphere, above 80° , nor a little below 60° . It never fails to disappear in Egypt, at the summer solstice,* the heat being then pretty uniformly at

* The same is mentioned by Bacon, as happening in Barbary. *Historia Naturalis*, Cent: iv.

80°, or upwards. Its chief prevalence therefore, is in Lower Egypt. It is almost unknown in Upper Egypt; not at all in Abyssinia, nor at Mecca, and the southern parts of Arabia. On the other hand, it appears from the history of all the plagues, of which there is any account in England, that they have never begun to appear, epidemically, but in the end of June, or beginning of July; that they proceed increasing till September, when they are at their *acme*, and then decrease till they entirely subside in winter, with the exception of a few sporadic cases.

The pestilential yellow-fever in like manner, has its own range of atmospheric temperature, but on a higher scale than the plague; for it cannot subsist long, if the thermometer falls below 80°. It has never been known in North America, nor in the South of Europe, but at the season of the year in which tropical heats, that is of 80°, or upwards, prevail, and it has never failed to disappear in winter, even in the mild winter of Spain, as already remarked. Nor has it ever appeared in those parts of Europe, where the summer heats do not rise to the tropical pitch. It has never been known in France,* nor England, and but once in Portugal. Its range has been circumscribed to Cadiz, in latitude 36° 25', the south coast of Spain, as far as Leghorn in

* Caillot mentions that it once merely made its appearance at Brest, and died away. The latitude is 48. 23.

Italy, in latitude, $43^{\circ} 33'$, the most northerly point which it has reached as an epidemic.

4thly, It has been further argued, by the non-contagionists, that, if it were really contagious, it would spread from the sea-port towns to the adjacent country, which it did not, either at Grenada, or in North America.* If it can be made good, as a matter of fact, that it is actually infectious in these towns, this argument can be of no avail; and it will be for those who delight in speculation, to exercise their ingenuity in accounting for its not extending to the country, by inventing some theory that will tally with the fact. But here too, we are not without the countenance of analogy, for something very like this happens in the plague. This great epidemic has a wonderful attraction to dense population, and squalid habits of life. It is this, together with the neglect of precautionary means, which at all times fosters, and perpetuates it in the Mahomedan towns; and in London, it always fell first on the indigent, and ill-aired quarters of the town. The Lord Chancellor Clarendon,† relates in the history of his own life, that when he quitted London, with the court, in the plague of

* It was observed at Barcelona, as well as in North America, that this infection did not spread among the rural population, with the exception of three persons, as stated in the Barcelona Report, p. 43.

† See Medical and Chirurgical Transactions, vol. iv. p. 102.

1665, and returned next year, he missed very few of his friends and acquaintances, who remained behind, the ravage having, in a great degree, been confined to the lowest orders of the people, who, in that age, lived in circumstances of great filth, and foul air. This fact is mentioned in still stronger terms in the account of the plague which prevailed at Copenhagen in the year 1711. It states that scarce one person of note died of it.* I am induced to suspect, and even to believe, that the exemption of London from plague ever since 1665, has been owing to the improved habits of life, which began on occasion of the great fire, the year after the plague, and have rapidly gained ground ever since. The commerce, and general intercourse with the Levant, as well as all other parts of the world, has been so much greater than in former ages, that it is difficult to believe, that particles of infection have not at various times been imported, but they have failed to take effect, for want of a suitable *nidus*, or fuel, as it were, to foster and kindle them.

5thly, They still further allege, that it is contrary to the common course of nature, that a disease, without any adequate or assignable cause, should thus visit these islands, at such long and uncertain intervals. This objection is much more applicable to that opinion which imputes this

* See Dr. Short's General History of the Air, vol. ii. p. 51. Lond. 1749.

disease to the exhalations of the soil, which being an act of Nature, ought to be much more steady than that which depends on the contingency of human events. But waving this, the like argument will apply to the visitation of hurricanes; for, it is beyond the reach of human sagacity, to say, why these islands should be more liable to them than any other portion of the globe; or why they should return at such uncertain and unequal intervals: far less can our philosophy detect upon what modifications of the atmosphere these convulsions of it depend. But shall we therefore deny the reality of hurricanes? This pestilential fever is the hurricane of the human frame, equally uncertain in its recurrence, equally dark and inscrutable in its cause, equally and deplorably certain as to the reality of its existence; but unequal as to its powers of destruction, if this is to be measured by the loss of human lives, for it has caused a greater waste of the species, than all the convulsions of nature put together, hurricanes, earthquakes, and inundations. Is it to be endured, that, by a piece of cavilling sophistry like this, a monster shall be unchained, which, in the course of a few years, has devoured more than half a million of human victims?

Lastly, The advocates of non-contagion seem to have been much influenced by the spirit of simplification or generalisation. This appeal to the uniformity of Nature, is answered by the fre-

quent reference we have made to the great difference between animated and inanimate Nature, as subjects of analogy, the latter being from its variety and complexity infinitely more liable to diversities and exceptions. It is on this principle that they have laid great stress on definition, holding that the term infection is not applicable but to those diseases which possess a contagious property in all circumstances of climate and seasons, of purity and impurity of air. To these the only answer must be, “Be it so; but are you, in order to indulge yourself in this miserable play of words, to sport with lives of thousands of your fellow-creatures?” This definition excludes the plague, and it will hardly be believed, that, in this age, there are persons pretending to medical education and science, who actually argue that this disease is not contagious. I am told there is a work of two quarto volumes recently published, the chief object of which is to maintain this extraordinary doctrine: and a work has lately been published at Strasburg, the object of which is to disprove the contagious nature of the venereal disease. No rational reader will expect the Author to enter into a serious refutation of such disgusting and extravagant paradoxes. We are compelled to apply to physicians, what one of the ancients has said of philosophers, *Nemo ægrotus quicquid somniat tam nefandum quod non aliquis dicat philosophus*; but with this material

difference, that the conceits and absurdities of philosophers, are generally harmless; whereas those of physicians may draw along with them the most serious calamities. This definition excludes the yellow fever, as well as the plague; and here that sophistry which is founded on ambiguity of language has opened another source of fallacy; for, they maintain also, that the term yellow fever can mean only one single disease. But ought the experience and common sense of mankind to surrender themselves, to any profusion of bewildering words, however confidently and imposingly pronounced, and spread through some thousands of pages? What can be so affecting and humiliating, as that persons of the plainest understandings should form sounder judgments on these important points, than those who value themselves on scientific attainments and research? Will not the world, without judging with its usual severity, be disposed to regard these our boasted attainments and researches, not as the legitimate lights which guide us in the avenues to truth, but as false lights, leading *us* into error, and *them* into danger; and tauntingly pronounce of our learned labours, that they only teach us *insanire ratione modoque*. Nor can it be matter of indifference to those, who feel for the interest and dignity of the profession, that any of us should become the objects of disrespect, and be exposed to the sneers of the extra-profes-

sional part of the community, by falling into errors, which lie open to the detection of the most ordinary and uncultivated minds.

But, according to the strict principle upon which this question ought to be decided, all these reasonings are absolutely nugatory, and only a waste of time : for, if it can be historically made out by legitimate evidence, as a matter of fact, that this disease in its epidemic form has never taken its rise but in sea-port towns, where it can, in most cases, be traced to the arrival of shipping conveying infection ; that it has never spread, but by contact, or near approach to the sick, and if it can be excluded by quarantine and separation from the sick, then do all discussions become vain regarding the resemblance of its symptoms to other diseases, or the previous state of the ship which imported it, or its communication being limited to a given temperature of the air, or to a given susceptibility of subjects ; and whether it is conveyed in the holds of ships, or on men's persons, and whether there is a marsh or no marsh in the vicinity. All these points are quite foreign to the subject, the simple question being, whether the disease is actually communicable from one person to another. It is only the matter of fact, as established by evidence, with which those civil and military authorities have to do, to whom is entrusted the sacred charge of the public health. These authorities perceiving the

jarring opinions of medical men, may, without deference or reference to them, undertake to judge for themselves, on a point to which any man of good sense and understanding is competent, as it hangs upon matters to be decided by the rules of evidence, not involving professional knowledge, the only matter of doubt being this essential property of the disease, its being personally communicable. And in case these authorities should wish for assessors to sit in judgment with them, they will probably deem it more safe and advisable, to ask the assistance of some members of the bench or the bar, accustomed to weigh evidence, and investigate facts, or even of such plain men as compose juries, than of medical men, having so much reason to suspect, that our minds are warped by prejudice, with our heads so overcharged with learning as to leave no room for common sense ; and so over-heated with contention, that we are more intent on victory than solicitous about truth.

Every one acquainted with human nature, knows how difficult it is for the mind to extricate itself from the shackles of prejudice, when riveted not only by time, and habit, but by that pride of opinion, which confirms, and perpetuates self-delusion, in opposition to the clearest evidence. But it is the duty of every liberal and considerate man to observe forbearance, and to judge with indulgence of weaknesses from which none of us

are exempt. Gentlemen, whom I know to be men not only of the greatest integrity, and honour, but of superior attainments, have advocated the cause of non-contagion, *bondâ fide*, and with intentions as pure as those of the opposite opinion. Nor is it easily conceivable, that any set of men, far less those of the medical profession, can be otherwise than sincere, or that they can mean positive mischief. If there have been persons who have stooped to the suppression of facts, and even to connive at perjury, as has been alleged, I should not impute even this conduct to bad intentions, but to that misguided and erroneous conscience, which by a perverted casuistry, deems pious frauds to be justifiable, and holds that a *little evil* may fairly be done, in order that a *great good* may come of it. All this ought to serve as a warning to the junior members of the profession against embarking hastily as the partisans of any doctrine, particularly such as involves some of the dearest interests of society, for in the course of time, they may be called upon to engage in a struggle, in which no man ought to be confident of his powers, namely, that severe ordeal of human virtue, by which they may be required to sacrifice the pride of opinion at the shrine of conscience by an abjuration of error.

It may be alleged by the partisans of the other side that, to those who advocate the cause of contagion, the imputation of a blind adherence to

inveterate prejudice is equally applicable. The fairness of this is not denied. Let the candid, impartial, and judicious arbiter of this question, whether among cotemporaries or posterity, judge betwixt us. Only let two important points distinguishing the two sides, be taken into consideration before the award shall be pronounced. The one is that the advocates of contagion have on their side the great advantage of *Moderation* ; for they concede that a great proportion of what is called yellow fever is free from contagion ; whereas the other party, or a great majority of them, will not admit even of a doubt regarding the truth of their opinion in all cases. The arbiters will no doubt consider on which side Catholic bigotry and infallibility are most conspicuous.—The other consideration is, that if there be the shadow of a doubt in any practical case, the safe side for the judges will be that of contagion ; for, whatever temporary loss or embarrassment may arise to commerce, or whatever inconveniences may be imposed on individuals, by personal restraint, these evils weigh as nothing in the balance against those of a spreading pestilence.

It is obvious, from what has appeared in the course of this discussion, that an accurate and ample history of a disease is requisite, for the purpose of prevention, as well as for that of cure. The errors we have been adverting to have arisen, in the first instance, from the want of a sufficiently

comprehensive knowledge of the subject. It was viewed on a local, partial, and narrow scale, instead of being surveyed in its whole extent, and various bearings. Accordingly, we see, that as knowledge has accumulated, error has vanished. In America, during the last years of the last century, the majority of opinions were in favour of non-contagion, and even public medical bodies gave their opinion on this side; but in the year 1805, the College of Physicians of Philadelphia, as a body, gave their opinion in favour of contagion, asserting that the evidence of this was as strong, as for that of the plague; and almost every member of that of New York, has publicly given the same opinion: such is the meliorating and maturing influence of time. The College of Physicians of London have given their verdict on the same side. *Commenta delet dies.* But such zeal has of late been exerted, and not without success, still to prop the opposite opinions, as to induce the Author to dwell upon the subject thus fully, earnestly, and repeatedly.

The question seems now to be brought to such a point, that we may venture to challenge any candid, intelligent, and unbiassed man, whether in or out of the profession, and whose mind is not indelibly imbued with the dogmas of non-contagion, to open his eyes, and deny that this disease is contagious; and if it be not, then has the Author of this discussion lost every faculty of

distinguishing truth from falsehood, of discerning light from darkness.

P. S. Since this work was put to press, the Author has met with a letter from Dr. Pariset, to Dr. Robert, of Marseilles, dated Montalegre, near Barcelona, the 27th of November, 1821. After expression of deep regret at the neglect of preventive means, and after adverting to the conversion of many of the advocates and abettors of the opinion of non-contagion, this eloquent, animated, and philanthropic physician concludes his letter as follows: " Their own acts confound them : the cry of so many victims overwhelms them : they are ashamed to live on the grave into which they have precipitated so many unfortunate beings. One moment of sincerity, one slight sacrifice of self-love, a shade of distrust in themselves, would have saved Catalonia a load of evils, and themselves from endless opprobrium and remorse. They hope to escape from opinion : but opinion overtakes and judges them, and even the vulgar who applauded them, now condemn them."

NOTE

REFERRED TO AT PAGE 140.

On the 16th of May, 1795, the Thetis and Hussar British frigates, cruizing off the Capes of Virginia, fell in with five French armed ships, and after a severe action of an hour and a half, three of these struck their colours: but from the disabled state of the frigates, only two could be taken possession of. They proved to be the Prevoyante and Raison. The latter had been employed at Guadaloupe as a prison ship, a situation of all others the most likely to engender and harbour a stock of infectious filth. It was found, on boarding this ship, that there had been great sickness and mortality among the crew from the yellow fever since she sailed, which was on the 25th of April. Great care was taken in shifting the prisoners on board of the Hussar, to remove only those in perfect health. It was found, that notwithstanding this precaution, the same fever began to spread among the crew of the frigate. On the 28th of May, the frigates and their prizes arrived at Halifax, but only a few of the sick were landed, for the inhabitants taking alarm, lest the infection should be introduced into the

town, it was determined, after a consultation of naval officers, that the remainder of the sick should be landed at some miles distance from the town, and accommodated in tents, and that the ships themselves should be put in quarantine. The like precautions were taken with regard to the sick prisoners. The number of sick landed from the Hussar, under the care of the surgeon of the ship, and his assistant, was eighty-three, all of whom recovered. The calamity which befel the men sent to man the prize, has been stated in the former part of this work, page 139.

The truth of this statement was denied by Dr. Bancroft, in a work published in 1811. His arguments are grounded on discoveries which he alleged he had made in documents deposited in the public offices in London. He perceived in the muster books at the Navy Office, that the whole of the prisoners' names were entered as victualled on board of the Hussar, to the number of 116; and that prize-head-money had been paid for the same number. From this he inferred that the whole of the French crew had been brought on board of the frigate; that there was not only no yellow fever, but no illness whatever among them, either on board of their own ship or in the frigate, for he understood that no men could be victualled but what were actually on board, and that all sick men were checked for their provisions; concluding from all this, that

the whole story of the yellow fever was a fable, invented by the surgeon of the Hussar.

Having myself served for four years as physician to the largest fleet that ever was employed on foreign service; and, having in that time been present in six general actions, these statements and assertions, so contrary to what I understood to be the rules of the service, greatly astonished and startled me. But lest my recollection should have failed me, or lest there should have been some change in the regulations, I addressed a letter to the Chairman of the Victualling Office, who having been a naval officer himself, was well acquainted with the practical rules, as well as the official business of the service. I also sent Dr. Bancroft's book to the office, begging them to make a report on that part of it which related to this subject. The Report is in the following words:

“ Prisoners of war of every description, whether
“ wounded, sick, or well; whether remaining on
“ board of their own ships, or transferred to the
“ capturing ship, are entered on the books of the
“ capturing ship, for two-thirds allowance of provisions. There is no exception to this, unless
“ when a prize is taken so near an English port,
“ as to be brought in the same day. It seldom
“ happens that all the prisoners are shifted; not
“ only the sick and wounded, but generally some
“ of those in health being left on board to assist

“ in working the prize, in consequence of the few
“ men ships of war like to part with on these oc-
“ casions. If the prisoners should fall sick, they
“ would not be checked for their provisions, and
“ no sick on board of their own ships are ever
“ checked of the allowance by the crown.

“ From this it will be clearly seen, that the in-
“ formation obtained by the person alluded to,
“ has been erroneous or misconceived.

“ P. S.—Muster-books are not to be considered
“ as infallible, as on occasion of captures great
“ confusion arises about men's names, particu-
“ larly of the prisoners left in the prize, who are
“ likely to be altogether omitted.”

It was therefore, on information, in every particular erroneous, that Dr. Bancroft ventured to charge Mr. Wilson, surgeon of the Hussar, with falsifying those statements, upon which I grounded my argument in favour of the contagious nature of the yellow fever.

There are a number of other inaccuracies of inferior importance charged upon him; and also on the surgeon of the hospital, to whom Dr. Bancroft will not allow even the faculty of eye-sight in discovering the colour of the men's skins. Many of these are likely to be real; and still more industry might have furnished still more aliment for captious cavilling; particularly had

Mr. Wilson so far forgot what was due to himself, as to have submitted to the endless humiliating interrogatories put to him. It would be strange indeed, if in ordinary circumstances inaccuracies did not occur on such occasions from hurry and confusion, but when the dismay of pestilence was added to that of war in some of its roughest forms, it would be incredible were any one to assert that inaccuracies had not happened. It does not seem to have occurred to Dr. Bancroft to make any allowance for these circumstances.

I ought not to omit, that soon after I heard of the truth of these transactions being denied, I called on Captain (now Sir John) Beresford, who commanded the Hussar. He assured me that Mr. Wilson's statement was correct in every point; adding, that having had the yellow fever himself, he well knew what it meant. He informed me further, that, after the sick were sent to the tents, he himself, his officers and men in health, went also on shore under tents at a prudent distance from the sick, and remained there till the ship was thoroughly cleansed, purified, and fumigated, the whole ballast being taken out, and the hold swept.

What now becomes of the vaunts of our author, exulting in the victoriousness of his cause,*

* This author's opinion of himself and his cause, will be readily inferred from the following copy of the introductory

and of the many encomiums of his partisans, proclaiming his statements to be infallible, and his arguments to be unanswerable? Were we to judge him with one half of the severity with which he has judged others, or were we to take this as a specimen of the reasonings with which his voluminous writings are swelled, what opinion should we form of the correctness of his facts and the accuracy of his research? I do not mean to arraign Dr. Bancroft of that wilful falsehood which he so unsparingly imputes to Mr. Wilson and others. He merely believed what he was told by a clerk in office, as ignorant as himself of the practical usages of actual service. I have here no personal exceptions, much less any hostile feelings. He has only questioned my accuracy and judgment, points of criticism, upon which all publick men are fairly accountable. He has been pleased even to mention my name with respect. But I here earnestly deprecate, nay, loudly protest, in my own name, and that of all those who

sentence to the sequel to his Essay on the Yellow Fever, published last year, 1818. “ In the year 1811, I published an “ Essay on the Yellow Fever; and, by *facts not to be invalidated, while truth continues to be invariable*, proved it to be no “ other than an aggravated form of that multifarious disease, “ which is well known to result from the action of those exhalations commonly denominated marsh miasmata, though “ often extricated from soils and situations, which are not “ marshy, &c.”

value the dignity and respectability of the profession, against those arguments on professional subjects, which consist in aspersions on moral rectitude: character being a possession dearer to every man of good principles and sentiments, than life itself; and calumny being, of all wrongs, the most difficult to repel, and that which admits the least of atonement or redress.*

In order to obtain still further information on this matter, I inspected the captains' journals of the *Thetis* and *Hussar*, deposited at the Admiralty Office; and also the account of the engagement, as detailed in the *London Gazette*, of the

* The high value here set upon honest fame, is no extravagant and romantic notion, derived from the ages of chivalry. The justness of the sentiment can be maintained not only on ancient classical authority, but it has the sanction of Holy Writ. The Roman satirist says,

Summum crede nefas animam præferre pudori,

Et propter vitam vivendi perdere causas.

And we find from the etymology of a scriptural word, the horror, in which the depriving any one of his good name ought to be held. *Διάβολος* derivatur a *Διαβάλλω*. calumnior. The verb in this sense occurs in the works of Xenophon, Demosthenes, and Aristophanes; but the derivative *διαβολος* is not to be found in any profane author, and is a word purely scriptural. This attribute is, I believe, by many serious persons deemed not to be the worst which enters into the character of this malignant being; but it would appear that the Evangelists thought it one of the most descriptive, by coining this new vocable by which to designate him.

27th of June, 1795. In the captain's journal of the *Thetis*, the number of prisoners in the *Hussar*, is stated at 130. In the captain's journal of the *Hussar*, they are stated at 125. We have seen, that the number stated in the muster-book of the *Hussar*, and of the charge for head-money, was 116. In the victualling-books, they are stated at 117.

Incorrect, and apparently inconsistent, as these various statements may be, they nevertheless admit of the following explanation.

The *Thetis* being the Commodore's ship, all the important documents belonging to the prizes, would be carried on board of her, and among the rest, the *rolle d'équipage*, containing the names and number of the men on board, when the ship took her departure from Guadaloupe, which was probably 130. The number on the captain's journal of the *Hussar*, was probably taken from the number, when the ship went into action, as nearly as they could ascertain, this being an important point, for it is the number on which head-money is strictly due. This was 125. The number on the muster-book, 116, was the number of survivors after the battle; part of whom, as usual, would be transferred to the victor, and part left behind, but all equally entered on the books, as already stated. Among those left behind, would certainly be the wounded, for that there were wounded as well as killed, it is impos-

sible not to believe ; for, it is stated in Captain Cochrane's letter, in the London Gazette, that the frigates did not open their fire, till they were within half musket-shot. And there is this further proof of the closeness and severity of the conflict, that the wounded bear a smaller proportion to the killed, than in any action I ever knew : the former, being in the Thetis nine ; and the latter, eight. The proportion of the wounded to the killed, is a sort of criterion of the distance of contending ships, on account of the number of splinters from spent shot, when they are less closely engaged.*

I should say, therefore, that the difference between the number at departure, 130, and the number in going into battle, 125, was the number of those who died on the passage, being five ; that the difference between the number which went into action, 125, and the number on the muster-book, 116 or 117, is the number killed in action, being eight or nine. With regard to the head-money, it is evident, the captors had not what was due to them, which was probably owing to its having been paid long afterwards in London, and the number being taken from the muster-books at the Navy-Office.

But it may be asked, why all this discussion regarding historical incidents ? Is there not sufficient internal evidence in this case, to repel the

* See Dissertation II. vol. i. p. 132.

charge? For, is it conceivable, that Mr Wilson, whose character is that of a man of honest simplicity of mind, could, without any assignable or adequate motive, become at once so flagitious, as to invent such a systematick machination; or had he been capable of conceiving such a purpose, would it have been possible for him to have blinded and deluded so many enlightened persons, professional and unprofessional?

It now only remains for me to vindicate myself, if I can, from an obvious charge which may be brought against me. It may fairly be asked, why, with my convictions on this subject, I should for seven years, have suffered the world to be imposed on, in a point of the highest importance, by statements, the fallacy of which I had ample means at any time of demonstrating.

In the first place, from my utter abhorrence of professional controversy, and having, through life, never publicly noticed the various obloquies, misconstructions, and contradictions, to which every man, in the exercise of his publick duties, is exposed, I was in hopes, at the end of my life, to be able to say, that I had never, through the medium of the press, taken part in any professional contention. I was perhaps vain enough to think that my character might have proved some sort of shield and answer to them.

If it should be said in reply, that this may be a very good rule in the case of private injuries; but

in cases, where the best interests of society, and the lives of myriads are eventually at stake, there is no room for pleading indolence and the love of peace, far less an over-weening conceit of character. To this I could only rejoin, that I trusted that before long, the native force of truth would operate and prevail, especially as the cause was supported by advocates more able than myself. And I believed a few years ago, that this golden era had actually arrived; for it did not occur to me, how any doubts on this subject could possibly remain on the mind of any rational being, after the publications of Sir James Fellowes, Mr. Pym, Sir Joseph Gilpin, and others. But I was mistaken. Much as I had studied the human mind, much as I have striven to measure the extent, as well as variety, of its aberrations in all their dimensions, I had not, it would appear, duly fathomed the length and breadth, nor sounded the depths of self-delusion, and the pertinacious pride of opinion; for, within these few months, there have appeared publications, as full of sectarian zeal, of plausible and imposing language, as at any period; and I am well assured, that they have even made converts. I should as soon hope to argue a bigot out of his belief of the most absurd tenet of his creed, as to change the opinions of the leading partisans of this doctrine.

To those who are not acquainted with the history of this controversy, it is necessary to offer a

still further explanation of the motives of the Author in thus republishing his original Dissertation in all its fullness, after saying, in the second edition, that in consideration of the opposition to the opinion of the existence of contagion having nearly ceased, he had greatly abridged* that discussion, and gladly suppressed all his animadversions on Dr. Bancroft's conduct. It has been seen, that the Author found himself grievously mistaken regarding the opinions and practices of the non-contagionists, not only in Europe, but in North America and the West Indies. It is notorious, that in the course of the two last years, many thousands of human beings have perished miserably at Barcelona and the Antilles by the neglect of preventive measures; and there are still professional persons in the United States of America, who hold quarantines to be unnecessary. There is also a peculiar incitement to the Author to republish the case of the Hussar frigate, for if it can be proved beyond a doubt, even in a single instance, as already adverted to, that the disease can be communicated from one ship's crew to another, in the middle of the ocean, the question is decided.

The Author has, in the whole course of this controversy, given the most unequivocal proofs of his extreme reluctance to engage in it; and wished more particularly to observe all possible

* The Article was reduced from 70 to 17 pages.

tenderness towards the individual who took the main lead in opposition to him ; and he is confident that this gentleman will be fully convinced, that nothing but the most over-powering dictates of duty could urge him to a measure which could not be omitted but by a compromise of conscience, and an insensibility to the eternal obligations of truth and humanity.

The sole purpose of this Dissertation has been to establish the reality of contagion. The Author has been discouraged from entering into questions of curative practice, by the great fallacy and inefficacy of all means that have been hitherto proposed, and he has none better to offer. But before taking leave of the subject, he will briefly advert to a few practical points.

The first he will mention is, that he has some reason to believe that a course of mercury, previous to entering the Carribean station, has the effect of removing, or at least of lessening the susceptibility to the yellow fever. The chief fact upon which he founds this, is what occurred in one of the ships of war which arrived in the West Indies towards the end of the American war, in which a great proportion of the crew had been under a course of mercury on the passage ; and it was remarked, that after these men arrived and were exposed to the same causes of sickness as the others, they were the only persons who escaped the fever of the climate. It would appear,

that the mercury had reduced their constitution to the standard of the coloured and Creole inhabitants, and of the seasoned Europeans. The Author has no experience of his own on this subject, and leaves it to others to pay such regard to it as their own judgment may dictate.

The other remark he has to make, respects certain violent methods of practice which have been followed, even at a very late period, in the tropical fevers. He alludes particularly to the severe depletory remedies of bleeding and purging, especially the former, which have been employed without that discriminating judgment which is indispensable in all sound practice. His attention has been very strongly drawn to this subject, by lately reading the detail of a most afflicting case of a young naval commander, on the West India station. He was seized a few weeks after his arrival with a continued fever, the symptoms of which were by no means violent or alarming, nor was he of an athletick frame. The practice followed was that of bleeding every day, from one to two pounds for the first four days of his illness, and two pounds two days before his death, which happened on the sixth day. The whole quantity of blood abstracted amounted to a gallon. He was also repeatedly and severely purged with salts and calomel. This is the revival of a practice, which had been tried and abandoned on account of its want of success, at the beginning of

the great epidemic in 1793. The Author does not mean here to explode depletory remedies in all cases; on the contrary, he is well assured, from his own experience, and that of others, that a single bleeding in the first day, or at furthest, the second day of attack, may be highly useful in the case of strong plethoric subjects newly arrived from Europe. It is to be lamented, that the most cautious and judicious employment of remedies have not made any sensible difference in the general rate of mortality in this most malignant disorder; but it is truly humiliating to reflect, that it should be increased by rash and undistinguishing practitioners. Will the junior Members of the Medical Service of the Navy forgive a veteran in that department, who has zealously devoted to it a large portion of a long life, for warning and imploring them not to give lightly into such outrageous and indiscriminate practices, as they regard the sufferings and value the lives of their fellow men who are entrusted to their care: not to mention what is above all price in the estimation of every good man—peace of mind, and a clear conscience.

DISSERTATION X.

*A Statement of Facts, tending to establish an Estimate of the true Value and present State of Vaccination.**

It is now twenty-one years since Vaccination was promulgated in this country by Dr. Jenner, and fifteen years since it began to produce a sensible effect in diminishing the mortality from small pox. In regard to the latter period, it is coeval with this society; yet, though no discovery in nature nor in medicine has been more important to the interests of humanity, nor any which has ever so rapidly and universally won the assent and practical adoption of mankind, there are no notices of it in our records, except in our second volume, in an article by Dr. Bateman, in which he relates a case of a mother who was affected with the small pox a second time, by being exposed to infection, from some of her own chil-

* The substance of this Dissertation was communicated to the Medical and Chirurgical Society in the year 1819, and published in their Transactions of that year. It was reprinted separately with some additions, in the year 1820, by the desire and at the expence of Dr. Jenner, who caused it to be distributed gratuitously by the publick establishments for Vaccine Inoculation.

dren, who had caught it casually, while her other children, who had been vaccinated, resisted it. As it is to be hoped that our labours will prove to posterity some of the principal sources of reference regarding the medical and chirurgical discoveries and improvements of the age ; as it is one of the reproaches of our country, that it has not availed itself so much as any other of the benefits of Vaccination ; and as there are writers among us who still allege that the failures are so numerous that the value of the discovery is very ambiguous, it seems one of the duties of this Society to lend its aid in placing these important points in their true light.

It seems almost needless to premise, that the small pox is of all maladies that, which, during the last thousand years, has destroyed the largest portion of the human species, and been productive of the largest share of human misery. There is, perhaps, no disease over which medical art has less power ; and this power, such as it is, has consisted more in abolishing pernicious practices, than in ascertaining any positive methods of controlling its fatality, unless we except the inoculation of it with its own *virus*. But, though the beneficial effect of this on those upon whom it is actually practised is undeniable, it has no tendency, like Vaccination, to extirpate the disease ; and from the impossibility of rendering it universal, it has actually been found to add to the

general mortality of the small pox, by opening a new source for the diffusion of its infection.

It ought to be stated also, with a view to a decision on this question, that Vaccination itself is attended with no danger, and frequently takes effect, without any visible disturbance in the system. There is even reason to believe, that in its process it wards off other diseases, by pre-occupying the constitution.*

* There is reason to believe that Vaccination saves from illnesses and death ; for if it were not so, the proportion of deaths of those under Vaccination, ought to be equal at least to those of the community at large ; nay, greater ; for it is generally performed on subjects during infancy, in which the proportional mortality is much greater than on the total population. This, however, is not the case, as may be proved from the Population Returns of the metropolis. The mortality of last year amounted there to 24,310 ; which, on a population of 1,274,800, gives a weekly mortality of 468. The population being divided by this, gives a weekly mortality of 1 in 2723 ; so that in this number of vaccinated subjects there ought to be, in the course of the weeks in which they are subjected to it, either one death, or one mortal illness, for death ensuing two or three weeks after Vaccination, would naturally be imputed to it. But as the mortality of children under two years of age, at which time they are mostly subjected to Vaccination, is somewhat more than one-fourth of the whole amount of mortality, there ought to die weekly considerably more than 1 in 2723. But as there are no reports of the mortality of vaccinated children which will justify this rate of mortality among them, nor any mortality at all, it follows, that death and disease are in less proportion than if they had not been vaccinated. And this is conformable both to reason and experience ; for it

In order to bring the general question to the test of calculation, in order also to institute a comparison of the mortality of small pox as affected by Vaccination, as well as by Inoculation from itself, I have selected from the bills of mortality four periods, each of fifteen years, for the purpose of exhibiting the mortality of small pox in each of these series in regard to each other. These are thrown into the form of Tables, and subjoined to this Dissertation.

The first series, is the fifteen years immediately preceding the introduction of inoculation; that is, from 1706 to 1720, both included. Previous to this period, no account that could be depended upon regarding the small pox, could be derived from the bills of mortality; for down to the beginning of last century such was their imperfect construction, that small pox, measles, and flux were blended under one head. Exception may be taken against the accuracy of these bills, even in this improved state, particularly with regard to the discrimination of diseases. This objection, however, is certainly less applicable to small pox than any other disorder, its character being so

is found, that the human body will not readily take on a double morbid action; so that a safe disease, like Vaccination, may occasionally avert a more severe and dangerous one. It is also believed in Ceylon and Mexico, as well as by some British practitioners, that Vaccination cures, as well as prevents, several disorders.

striking as not to be mistaken by the most ignorant and careless observer.

The second series is taken at the middle of the last century, when inoculation had made considerable progress; that is, from 1745 to 1759, both included. In comparing this with the preceding series, with regard to absolute numbers, it ought to be taken into account, that eleven parishes were added to the bills of mortality, between the years 1726 and 1745, both included: so that the progressive improvement of general salubrity ought to be estimated still higher than what is indicated by the diminished mortality, as it stands in the Tables.

The third series comprises the fifteen years previous to the introduction of Vaccination, when inoculation had made still greater progress; that is, from 1785 to 1798, both included.

The fourth series comprises the time in which the vaccine inoculation has been so far diffused as to produce a notable effect on the mortality of small pox, that is, from 1804 to 1818, both included.

The result of these computations stands as follows:

Ratio of the Mortality of Small Pox, to the total Mortality.

From 1706 to 1720, one in 12.7; that is, 78 in 1000.

From 1745 to 1759, one in 11.2; that is, 89 in 1000.

From 1785 to 1798, one in 10.6; that is, 94 in 1000.

From 1804 to 1818, one in 18.9; that is, 53 in 1000.

Fractions are not noticed in the last column of numbers.

It appears from this statement, that the proportion of deaths from small pox to the total mortality, increased in the course of last century; so that inoculation seems to have added to the mortality. It is but fair to mention, however, that this total mortality is not quite a just scale whereby to measure the relative mortality of small pox; for in the course of that century, the general mortality itself was greatly diminished in relation to the population. This diminution of general mortality was chiefly owing to the diminished mortality in children under two years of age, which, at the time when the account began to be kept, 1729, averaged about 9000; but at the end of the century not more than 5000;* also to the decrease of fevers, and still more of fluxes.

* This diminished mortality of young children is, like that of fevers and fluxes, owing chiefly to the improvements in ventilation and cleanliness, but greatly also to laying aside the custom of exposing them to the open air in winter and early in spring; either from inadvertency, or from the false notion of rendering them hardy, whereas they thereby catch inflammations of the lungs. Nothing tends more to the health, strength, and growth of children than genial warmth. It seems chiefly owing to the great plenty and cheapness of fuel, that the race of people in Lancashire is so superior in their form and size. In Buckinghamshire, on the contrary, where fuel is extremely scanty and dear, the race of people is small and puny, insomuch that it is provided by Act of Parliament that men shall be admitted into the militia of a smaller stature in this than other counties. This complaint has of late years been greatly alleviated by the inland navigation conveying coals by canals.

The relation of the mortality of small pox to the population, would therefore be a more fair criterion of its increase or decrease. In this view it might, at first sight, be thought that it had decreased: for the population of the metropolis nearly doubled in the course of the last century. But it is to be remarked, that there has been little increase of population in that portion of the metropolis which is included in the bills of mortality; the great increase having been in the parishes of Mary-le-bone and St. Pancras, which are not included in these bills. It is computed in the remarks subjoined to the last parliamentary returns of population, that the population of London, within the walls, had decreased more than three-fifths in the course of last century, from the widening of streets, the erection of public buildings and warehouses, and, it might have been added, from the migration of mercantile families to the west end of the town. As a set-off to this, there has certainly been a great addition, in the same time, to those parishes within the bills, which stand on the verge of the metropolis, such as St. George's Hanover Square, St. George's Bloomsbury, Poplar, and Stepney. But the addition to the population, if any, within the bills of mortality, does not seem to be so considerable as to affect the computation. And, if this is admitted, the absolute numbers of the deaths from small pox, estimated in relation to the population,

that is, exactly as they stand on the Tables, afford a fair comparative statement of the mortality in the last century, and seem to prove that inoculation has not added so much to it as has been alleged. It was in the rural population that the effect of inoculation in diffusing small pox was chiefly felt. In this situation there is much less intercourse of persons with each other than in towns, so that not only many individuals escaped from their not being exposed to infection during their whole lives, but whole districts were known to have been exempt from it for a long series of years, before it was universally diffused by inoculation.

But the truly important result from these statements consists in the clear, undeniable, and great diminution of it since the Introduction of Vaccination. It appears, that in the last fifteen years, the mortality from small pox, in the bills of mortality, has not been much more than one-half of what it was in the two like series of years in the middle and latter end of the last century. Nor does this comprise the whole benefit derived from this discovery in the metropolis; for, besides that the sixth part of it lies without the bills, it was found, in levying the tax on burials for the last six months of 1794, that the number of unregistered deaths, chiefly those of dissenters, amounted in that half year to 3148; and the reporter of the parliamentary enumeration thinks that, as besides

these there were undiscovered interments, the unregistered deaths may be computed at one-third of the total mortality, that is, about 7000. (*See Abstract of the Parish Registers, 1811, printed by authority of Parliament, page 200.*)

Assuming, therefore, that Vaccination had not been practised the last fifteen years, and that the mortality from small pox, within the bills, had in that time, that is, from 1804 to 1818, been the same as from 1784 to 1798, that is 27,569 in place of 14,716; and assuming that there has been the same proportional diminution of deaths in the districts without the bills, and among the unregistered subjects, the account of lives saved in this metropolis by Vaccination in these fifteen years, will stand as follows:—

Within the bills of mortality	. 12,853
Without the bills of mortality	. 2,570
Unregistered cases 7,711
Total	<hr/> 23,134

The first of these numbers is found by subtracting the amount of deaths by small pox, in the bills of mortality, during the practice of Vaccination, from the amount of them during the same number of years, immediately before the discovery of Vaccination.

The second number is found by dividing the first by 5. The population of the metropolis without the bills is stated at one-sixth of the

whole, which is evidently one-fifth of that within the bills.

The third number is found by dividing the sum of the two others by two; the unregistered cases, being, as before stated, one-third of the whole.

It appears, therefore, that even under the very imperfect practice of Vaccination which has taken place in this metropolis, 23,134 lives have been saved in the last fifteen years, according to the best computation that the *data* afford. It will be seen, by an inspection of the Table, that in that time there have been great fluctuations in the number of deaths. This has been owing partly to the small pox inoculation of out-patients having, by an unaccountable infatuation, been kept up at the Small Pox Hospital for several years after the virtue of Vaccination had been fully confirmed. The greater number of deaths in 1805 may chiefly be referred to this cause. Since the suppression of this practice, the adoption of Vaccination, though in a degree so incomplete, in consequence of public prejudice, created entirely by mischievous publications, has been unable to prevent a considerable, though fluctuating, mortality from small pox. The late mortality from small pox, though little more than one-half of what it was in former times, might have been entirely saved, if Vaccination had been carried to the same extent as in many cities and whole districts on the continent of Europe, in Peru, and Ceylon.

In the summer of 1811 the Author was called to visit professionally, Don Francisco de Salazar, who had arrived a few days before in London, on his route from Lima to Cadiz, as a deputy to the Spanish Cortes. He informed him, that Vaccination had been practised with so much energy and success in Lima, that for the last twelve months there had occurred, not only no death from, but no case of, small pox; that the new-born children of all ranks are carried as regularly to the Vaccinating House as to the font of baptism; that the small pox is entirely extinguished all over Peru; nearly so in Chili; and that there has been no compulsory interference on the part of the government to promote Vaccination.

It is now matter of irrefragable historical evidence, that Vaccination possesses powers adequate to the great end proposed by its meritorious discoverer, in his first promulgation of it in 1798, namely, the total extirpation of small pox. The first proof of this was at Vienna, where, in 1804, no cases occurred, except two strangers, who came into the city with the disease upon them. In 1805 there did not occur a single death from it in Copenhagen.* Dr. Sacco, the indefatigable superintendent of Vaccination in Lombardy, stated, in his annual report, 3rd January, 1808, that the small pox had entirely disappeared in all the large towns in that country; and that in the great city of Milan it had not appeared for

* See Pfaff Neuen Nord v. Archiv. B. I.

several years. Dr. Odier, of Geneva, so favourably known for his high professional, scientific, and literary acquirements, testifies, that, after a vigorous perseverance in Vaccination for six years, the small pox had disappeared in that city and the whole surrounding districts; and that, when casually introduced by strangers, it did not spread, the inhabitants not being *susceptible*. The Central Committee in Paris testify, in their Report of 1809, that the small pox had been extinguished at Lyons and other districts of France.

These are selected as some of the earliest and most remarkable proofs of the extirpating power. But it is demonstrable, that if at the first moment of this singular discovery, at any moment since, at the present or any future moment, mankind were sufficiently wise and decided to vaccinate the whole of the human species who have not gone through the small pox, from that moment would this most loathsome and afflicting of all the scourges of humanity be instantaneously, and for ever, banished from the earth.

It is further manifest, that extirpation being the ultimate aim of this discovery, and there being the fullest historical and practical evidence of its being capable of accomplishing this end, all other questions with regard to its expediency must be futile and irrelevant. It is in the nature of all morbid phenomena to be liable to exception. One of the most essential and characteristic

laws of small pox itself, namely, that of its affecting the human subject but once in life, is found in rare cases to be violated. It is, therefore, perfectly conformable to analogy, and naturally to be expected, that it may not in all cases be a complete security against small pox. But it is obvious, that, admitting these exceptions to be very frequent, much more so than the recurrence of small pox after small pox, this can constitute no objection to the practice, as long as the extirpating power remains unimpaired and unimpeached. Nay, it is obviously so far from an objection, that it ought to operate as a powerful additional incentive on every benevolent mind, to push Vaccination to the utmost, as rapidly as possible, in order that those who are still susceptible, either from peculiar natural constitution, or from the unskilful manner of conducting the operation, or from defective matter, may not, by any possibility, catch it; for, in the event of its extirpation, it could nowhere be met with. And in order to stimulate the good and the wise to aim strenuously at this consummation, let it be constantly borne in mind, that the adversary they are contending with is the greatest scourge that has ever afflicted humanity. That it is so, all history, civil and medical, proclaims: for, though the term plague carries a sound of greater horror and dismay, we should probably be greatly within the truth in asserting, that small pox has des-

troyed a hundred for every one that has perished by the plague.

It is true that in its last visitation of this metropolis, one hundred and fifty-four years ago, it carried off 70,000 victims in a few months; but since that time, the deaths from small pox, recorded in the bills of mortality, have amounted to more than 300,000; and a like number of the survivors have been afflicted with blindness, deformity, scrofula, or broken constitutions, which is not the case with the survivors of the plague. It appears, by a report of the hospital for the Indigent Blind, that two-thirds of those who apply for relief have lost their sight by the small pox. It is alleged by some of the soundest Political Economists that small pox does not diminish the numbers of mankind, nor Vaccination increase them; for population is determined by subsistence, and the indefinite powers of procreation soon repair the ravages of disease. But, however true this may be, the miseries incident to so many of those who survive small pox, whereby they become a burden to themselves, their families, and to society, render this disease incontrovertibly an evil of the first magnitude, not to mention the intense sufferings and afflictions inseparable from it; and in this view of the matter the objection adduced against Vaccination by one of its opponents,* that *small pox is a merciful dis-*

* See Serious Reasons for uniformly opposing Vaccination. By John Birch. London, 1807.

compensation of Providence for the poor man, by diminishing the burden of his family, will not hold good, for the burden is not removed.

And when it is considered that there are large portions of the globe, India, China, even one whole quarter of it (North and South America), besides all the tropical and arctic regions, in which the plague has never been known; and that in all the countries liable to it, it seldom appears but at one season of the year, and in some at long intervals, the ravage which it makes is trifling when compared with the unceasing havoc of small pox, which spares no nation in any climate, or at any season. Yet the Legislative regulations for excluding and checking the plague are of the the most harsh and despotic description, while the law touches upon small pox comparatively with the most lenient hand. It ought to be generally known, however, that in a late trial and conviction, it was laid down by the judge to be the law of the land, that a medical practitioner who neglects to exclude the person whom he inoculates from communication with others, is liable to fine and imprisonment. Morally considered, indeed, it is difficult to conceive a higher degree of flagitious turpitude than that of a professional person, in the present state of knowledge, exposing his fellow creatures, from sordid motives, to one of the most grievous calamities of which human nature is susceptible.

The preceding reasoning is grounded on the

supposition of extirpation : but, however demonstrable the *possibility* of extirpation may be, it may not in all communities be *practicable* ; and may not these alleged failures so operate, as, in such circumstances, to render the expediency of the practice questionable ?

In order to decide this, let the nature and amount of these failures be ascertained and estimated.

The description of those cases of small pox, (if they can be called so), which occur in vaccinated subjects, is shortly as follows :—The invasion and eruption in every respect resembles that of the genuine small pox. I have seen it attended with high fever and a thick crowded crop of *papulæ*, such as precedes the most severe and dangerous cases of the confluent kind. This runs on till the fifth day from the eruption, both days included, at which time some of the *papulæ* begin to be converted into small sized pustules. The disorder then abruptly stops short. On the following day the fever is found to have subsided, with a shrivelling and desiccation of the eruption, and recovery proceeds without the least danger or inconvenience. The face is marked, for some time after, with brown spots, but without pits. It should never be forgotten, that all morbid *phenomena* are full of varieties and exceptions. Accordingly, though the fifth day is the most common limit of this disorder, it sometimes stops

short on the third ; sometimes not till the sixth or seventh ; and, in a very few cases, it has been known to run the common course of small pox. What forms the strong line of distinction from proper small pox, is that, with a few exceptions, it does not advance to maturation and secondary fever, which is the only period of danger. I am not prepared to deny that death may not have occurred in a few instances ; nay, there seems sufficient evidence that it actually has ; but these adverse cases are so rare, as not to form the shadow of an objection to the expediency of the general practice. A few weeks ago, at a meeting of this Society, at which forty members and visitors were present, I put the question whether any of these eminent and extensive practitioners had met with any fatal cases of this kind. Two gentlemen had each seen a single case, and two other gentlemen took occasion to say that they had each seen a case of second small pox, both of which proved fatal. It is evident, therefore, that according to that maxim which guides mankind in the conduct of life, namely, that of acting on a general rule and average, and not on exceptions, these adverse instances ought not to have the least influence on practice, even though they were much more numerous, and though every one of the alleged failures were authentic cases of real small pox. Nor indeed do they, except in the very rare cases here cited deserve the name of failures ; for,

though they fail in preventing *small pox*, they do not fail to prevent *death*. And let me here, in the name of humanity, beseech practitioners not to be forward in publishing single cases of failures, whether real or supposed; for, when the weak minded and uninformed hear of these failures, without hearing at the same time that there are hundreds of cases of permanent security for every single case of failure, they are guided by the *exception*, which becomes to them the *rule*; their judgments being thereby most fatally perverted.

As it is of the utmost consequence to establish the strong and important distinction between small pox, properly so called, and that which takes place after Vaccination, which may be called the imperfect, or five day small pox, a few of the most impressive testimonies respecting the safe nature of the latter may be here recited. Mr. Brown,* of Musselburgh, gives the detail of forty-eight cases, in none of which did the secondary fever nor death occur. Here was a saving

* See Inquiry into the Antivariolous power of Vaccination. ed. 1809. There is an Article in the Edinburgh Medical Journal by the same gentleman in 1819, in which he mentions that he had heard of several deaths having occurred from cases of small pox after Vaccination. But, admitting this, it is utterly incomprehensible by what process of reasoning Mr. Brown could, on such premises, arrive at the conclusion that Vaccination ought to be exploded and abandoned.

of at least eight lives, at the lowest computation ; for this is the number which, by the average mortality of natural small pox, would have died if the constitutions of these forty-eight persons had not been modified by previous Vaccination. Dr. Dewar of Edinburgh, hearing that many vaccinated subjects had been affected with small pox at Cupar in Fife, where the natural small pox at the same time prevailed, most laudably repaired to the spot to investigate the subject. He found that fifty-four vaccinated subjects had caught the small pox. All these, except one, had the imperfect or five day eruptive fever, and lived. The fatal case was that of a child, who had a complication of other disorders, and having died on the fifth day, the small pox, according to its ordinary course of fatality, could not of itself be the cause of death. All the rest were safe ; while of sixteen cases of the natural small pox at the same time and place, six died ; so that, if these fifty-three cases had not undergone the mitigating process of Vaccination, nineteen or twenty would have perished. Between thirty and forty cases of the same kind have occurred at Carlisle, on the testimony of Dr. Barnes, a respectable practitioner of that city.* Many proofs might be adduced from the oral testimony of private practi-

* See also a clear and able exposition of this subject in the Medical and Surgical Journal for July 1818, by Mr. Dunning, of Plymouth.

tioners, which would over-swell this article. The only other to be mentioned is from the report of the Central Committee of Vaccination at Paris, made in December last, in which the description of the disease occurring after Vaccination corresponds exactly with the imperfect five day cases which have occurred in Britain. They refuse the name of small pox to it; but as I know from my own observation, as well as from the testimony of others, that the matter from it does by inoculation give the small pox, we can hardly, perhaps, with propriety, deny it that name; but it should be distinguished by some strong discriminating epithet, such as is suggested above.

In order to place the whole of this in a clearer view, I shall here as succinctly as possible enumerate the most remarkable irregularities of small pox belonging to itself, and in the mutual relation of it with its kindred exanthematous affections, cow pox, and chicken pox.—First, the great difference between one case and another of small pox, whether casual or inoculated, in point of mildness and malignity, depending on the difference of individual constitution or idiosyncrasy, as it is technically called.

Secondly, The liability of a few constitutions to suffer a recurrence of it a second time, and even oftener.*

* This has been no where that I know so well traced and ascertained as in a tract of great erudition, by Dr. Andrew

To these renewed attacks I would apply the epithet, *second*, or *recurrent*, in preference to that of *secondary*, which implies, if I mistake not, some difference in kind, and not merely repetition or recurrence, as is the case here; for these second attacks are cases of as genuine small pox, as those of the first attack. There is only this remarkable difference in these cases from those of ordinary small pox, that they are above the average rate of severity which takes place in first attacks. And it is further remarkable, that those first attacks which have sooner or later, in very rare instances, been succeeded by a second attack, have also been cases of uncommon severity, the contrary of which might be expected *à priori*. I ventured to mention this from my own observation* some years ago, and I find it confirmed by the very ample records quoted by Dr. A. Smith.†

Thirdly, The great abatement of malignity by the inoculation of it with its own *virus*. The title *mitigated*, seems most applicable to this kind of small pox, rather than that of modified or imperfect, for it is identical in its specific nature with that which is caught casually, the whole

Smith, subjoined to Dr. John Thomson's Historical Sketch of Small Pox. Edinb. 1822.

* See an Article on Infection, in the 3rd volume of Transactions of the Society for the Improvement of Medical and Surgical Knowledge. Lond. 1812.

† See further remarks on this subject, vol. i. p. 311.

series of phenomena being the same, but on a lower scale of intensity.

Fourthly, There is a sort of small pox which not unfrequently occurs, sooner or later after Vaccination, to which I would affix the epithet of *imperfect*; for though it is perfectly similar to the ordinary small pox in its first attack, it stops short at the termination of the eruptive fever, not proceeding to the dangerous stage of complete eruption and secondary fever. Its identity, however, is ascertained by the matter taken from the incipient and immature pustules, producing the ordinary small pox as perfectly as the ripe matter taken from genuine small pox. Of this I have had personal observation. And here it is of the utmost importance to remark, that that deviation from the ordinary course of nature, whereby the vaccine inoculation does not in all cases afford complete protection, is sufficiently accounted for by those exceptions, varieties, and peculiarities, so eminently displayed in all the phenomena of animal life, particularly in the diseases incident to the human frame, and above all, in whatever relates to infection. This consideration, while it puts an end to all the wrangling and angry feelings among the members of the medical profession, as exemplified in their contests respecting spurious matter, and errors in the operation being the causes of failure, tends to establish unanimity and satisfaction in the community at large, by

removing all vain alarms. Nothing inconsistent with truth and candour can be substantially and ultimately serviceable to any cause ; and here it is required that we should not only admit, but avow and proclaim, that Vaccination is, in many constitutions an imperfect protection against small pox infection ; but it ought equally to be promulgated, that the small pox occurring after Vaccination, is so rare, and at the same time so safe, as to furnish no objection to the practice. The publick ought also to be told, that though all the cases of failure, multiplied and exaggerated as they have been by ignorance, selfishness, and malignity, were strictly true, even this would prove no solid objection ; for thousands of cases of complete protection could be adduced for every single case of alleged failure ; and though a case of malignant and fatal small pox should occur after the most perfect Vaccination once in ten thousand cases, it is only what might be expected, and even might have been predicted, as an incident in accordance with the nature of small pox infection. And it will not be disputed, that this loss of one life is compensated by the protection afforded to 9,999 others, of whom 1,666 would have died, had they been affected with casual small pox, one in six being the lowest computation of its mortality.

Fifthly, and lastly, The other form in which small pox is alleged to shew itself, is that of Chicken

Pox, and other anomalous eruptions. From these making their appearance only, or chiefly, at times and in places in which small pox is epidemic, it is believed that they depend on the influence of variolous infection, acting on constitutions under particular circumstances of susceptibility. This has been ably investigated by Professor John Thomson, of Edinburgh ; but as it is a subject of great intricacy and obscurity, and as the sole object of this Dissertation is to exhibit a popular and practical view of the subject, in as concise a form as possible, the Author must be excused from dilating on this point, though it is one of considerable curiosity and interest. He does not mean to deny that it belongs to the kindred of small pox ; but there is one circumstance, namely, its not communicating that disease by inoculation, which places an insuperable line of demarcation between it, and those which have been above styled the recurrent mitigated, and imperfect forms of this disease.*

The only further remark to be made on this part of the subject is, that when the presence of small pox does not render prompt Vaccination advisable, it would be expedient to defer it in subjects labouring under herpetick and impetiginous affections, as it is apt in such cases to excite

* See the subject fully treated by Professor Thomson, with his characteristick industry and acuteness, in his account of the Varioloid Epidemic. Edinb. 1820, and his Historical Sketch of Small Pox, Edinb. 1822.

anomalous eruptions. It is also worth while to mention, that there is a simple and ingenious method of being assured of the perfect state of the vaccine operation, proposed by Mr. Bryce, of Edinburgh. This is to inoculate the patient on the other arm from his own matter, about the fourth or fifth day of the pustule. If this second inoculation produces a hurried and irregular pustule, it is a proof that the first inoculation has had its due effect. But if the second insertion produces a regular eruptive process, it is a proof that the first had failed in its effect.

As the attack of small pox in subjects who have undergone Vaccination, generally occurs after a long interval, it becomes a question, whether this is owing merely to the chance of such subjects not having been exposed to variolous contagion, or to the effect of time in diminishing the anti-variolous virtue of Vaccination. The former is certainly conceivable; but when we consider the numberless severe proofs to which the recently vaccinated were experimentally exposed in the early part of this practice all over Europe, from which the most satisfactory evidence resulted; and when it is considered that, in the great majority of cases, small pox has not occurred till several years after vaccination, it seems by far most probable that the virtue of it is weakened by time. When parents, therefore, become anxious and apprehensive regarding the risk of small pox after a lapse of years, it seems

quite reasonable that they should be indulged in having the operation repeated.

Let all this be applied to the case of a community, in which the total eradication of small pox is quite hopeless. Let it be admitted that such occurrences as have been described do frequently take place : let it even be admitted, for argument's sake, that every vaccinated case whatever must of necessity and unavoidably at some time or other in future life be affected with this mitigated and imperfect species of small pox, would it not even, under this great abatement, be one of the greatest boons that could be conferred on humanity, being an instrument or remedy which would disarm small pox of its danger? Would not the next greatest benefit to the total extirpation of small pox, be the stripping it of its terrors by rendering it safe and harmless?

It may be further remarked, that the benefit derivable from the different proportions of the persons vaccinated to the total population, advances in a considerably higher progression than the simple arithmetical. It is evident, that the smaller the relative number of the vaccinated, the greater their chance of meeting with small pox infection; and that though the disease which they may catch is of a very safe nature, it would nevertheless be desirable to avoid it on its own account, but still more on account of the prejudice it creates. This, in the eye of general bene-

volence, constitutes an additional, though secondary motive for extending the vaccine inoculation as widely as possible, even though the attainment of the *maximum*, that is, total extirpation, should be impracticable and hopeless.

It is of the highest importance to society, that this subject should be seen in its true light, and in all its bearings; for the frequent occurrence of these cases of small pox, however safe in themselves, has had a most pernicious effect on the credulous and ignorant, by giving a check to the practice of Vaccination. It ought never to be forgot, that the power of Vaccination in extirpating small pox being established, the question of its expediency is completely set at rest. How many parents are there now who, from a weak distrust in the virtue of Vaccination, have to lament the loss of a child from small pox, either casual or inoculated? Many such are known to myself. It is pleasing however, to observe, that though this unmerited discredit into which Vaccination had fallen, swelled the number of deaths in London from small pox to 1051 in 1817, good sense is likely still to prevail, for last year (1818) the deaths have fallen lower than they have ever been known since the institution of the bills of mortality, the total number being only 421.*

* Since the first publication of this Tract, it has appeared that in the succeeding year (1819), the deaths from the Small Pox had advanced to 712; which ought to add to the perse-

On the whole matter, I believe I am speaking the language of every man of good principles and feelings, capable of reflecting seriously and considerately on the subject, when I say, that whenever he applies his mind to it, he finds some new and increasing causes of complacency and satisfaction. Viewed as a mere physical fact in the natural history of the animal kingdom, the virtue of the vaccine *virus* in resisting the action of the *variolous*, is, by its novelty and singularity, highly striking and interesting to every one whose taste leads him to take delight in contemplating and exploring the devious ways and varied courses of Nature, as curious exceptions to the uniformity and constancy of her laws. One can hardly contemplate with sufficient astonishment the extraordinary fact, that a morbid poison taken from a domestic animal should, when inserted into the human body, shield it against the assault of one of the most fatal and cruel maladies to which it is liable. But the importance of this, as a physical curiosity, vanishes to nothing, when the unexampled benefits of it to mankind are fairly weighed; benefits which could never have been dreamt of by the most sanguine philanthropist, who, in contemplating it, finds himself lost in astonishment, as a boon to mankind almost beyond the grasp of his mind duly to appreciate: verance, zeal, and vigilance, of the friends of humanity in prosecuting Vaccination.

so that what seems at first sight merely a sportive aberration from the usual course of things, has, by the wise dispensation of Providence, become subservient to the most beneficent purposes : and how many more useful discoveries may there yet be in reserve for the alleviation of human misery, from obscure and undetected facts still lurking under the very surface of nature ? It will in the eyes of future ages be deemed an *epocha* in the destinies of the world, and one of the highest boasts of the country in which it took its rise, with a sense of unrequitable obligation to the individual* who first disclosed and promulgated the secret, by drawing it from the dark recesses of rural tradition, and rendering it available to the whole human race.

Such are the sentiments which must fill every well constituted mind ; and it behoves the whole medical profession, which has already done itself so much honour by the zealous and disinterested encouragement afforded to it, to continue its efforts in eradicating every remaining prejudice against it. It becomes Englishmen, in particular, to cherish it, not only as the native offspring of their country, of which they have reason to be proud, but to redeem the character of the nation from the reproach of having, of all others, whether savage or civilized, done the least justice to this noble discovery. It is somewhat humiliating

* Dr. Edward Jenner.

to reflect, that while there is no country which has received more striking and unambiguous benefits from this discovery, there is none which has prized it less, nor availed itself of it so little. I here allude to the unspeakable advantage of it to the public service, both by sea and land, in the late war, so eventful and portentous in its course, and so glorious in its termination. Formerly, small pox was one of the greatest embarrassments to the operations of armies; and ships of war were occasionally under the necessity of quitting the sea, from the prevalence of this disorder among their crews. Those lately at the head of the navy and army, with that vigilant wisdom and humanity which become those who direct the affairs of a great and enlightened nation, recommended and enforced the practice of Vaccination in both these departments, to the great furtherance of the public service. Their example has by no means been followed among the civil population of England. This is chiefly imputable to the abuse of the press, the general licentiousness of which may be denounced as one of the most grievous evils of this age and country, in regard to other subjects interesting to humanity and public happiness, as well as this; the votaries of error and depravity being more successful, because they find more encouragement in disseminating their principles, than the advocates of truth, virtue, and good order. There is no maxim

more true, than that the best things do become by abuse the worst, and that in proportion to their excellence. What a mortifying contrast does England form with Peru, where it was adopted instantly, in consequence of a flash of conviction from the light of evidence! And was not this conviction fully justified by the immediate disappearance of small pox from that whole region? To those nations who may feel an envy of the glory attached to our country by this discovery, it must be no small consolation to perceive that a large proportion of the English nation has hitherto been so besotted as not to know how to appreciate it, nor how to avail themselves of it, and that it has encountered more opposition among ourselves than in all the world besides.

TABLE I.

Years.	Total Mortality.	Mortality from Small Pox.	Proportion.	Proportion to 1000.
1706	22,097	1094	1 in 20	50
1707	21,600	1078	1 20	50
1708	21,291	1687	1 $12\frac{1}{2}$	79
1709	21,800	1024	1 21	49
1710	24,620	3138	1 8	127
1711	19,833	915	1 $21\frac{1}{2}$	46
1712	21,198	1943	1 11	92
1713	21,057	1614	1 13	77
1714	26,569	2810	1 $9\frac{1}{2}$	106
1715	22,232	1057	1 21	47
1716	24,436	2427	1 10	100
1717	23,446	2211	1 $10\frac{1}{2}$	94
1718	26,523	1884	1 14	71
1719	28,347	3229	1 $8\frac{3}{4}$	114
1720	25,454	1440	1 $17\frac{1}{2}$	56
Total 350,503		27,557	1 12.7	78

In this series it appears that the deaths from Small Pox are, to the total mortality, as 1 in 12.7; that is, 78 in 1000.

TABLE II.

Years.	Total Mortality.	Mortality from Small Pox.	Proportion.	Proportion to 1000.
1745	21,296	1206	1 in $17\frac{3}{4}$	56
1746	28,157	3236	1 $8\frac{3}{4}$	115
1747	25,494	1380	1 $18\frac{1}{2}$	54
1748	23,869	1789	1 $13\frac{1}{2}$	75
1749	25,516	2625	1 $9\frac{3}{4}$	103
1750	23,727	1229	1 $19\frac{1}{4}$	52
1751	21,028	998	1 21	48
1752	20,485	3538	1 $5\frac{3}{4}$	172
1753	19,276	774	1 25	40
1754	22,696	2359	1 $9\frac{1}{2}$	104
1755	21,917	1988	1 11	91
1756	20,872	1608	1 13	77
1757	21,313	3296	1 $6\frac{1}{2}$	155
1758	17,576	1273	1 $13\frac{3}{4}$	73
1759	19,604	2596	1 $7\frac{1}{2}$	132
Total 332,826		29,895	1 11.2	89

In this series it appears that the proportion of deaths from Small Pox is, to the total mortality, as 1 in 11.2; that is, 89 in 1000.

TABLE III.

Years.	Total Mortality.	Mortality from Small Pox.	Proportion.	Proportion to 1000.
1784	20,454	1210	1 in 17	59
1785	18,919	1999	1 9 $\frac{1}{2}$	106
1786	20,445	1210	1 17	59
1787	19,349	2418	1 8	125
1788	19,697	1101	1 17 $\frac{3}{4}$	56
1789	20,749	2077	1 10	100
1790	18,038	1617	1 11 $\frac{1}{4}$	89
1791	18,760	1747	1 10 $\frac{3}{4}$	93
1792	20,313	1568	1 13	77
1793	21,749	2382	1 9	11
1794	19,241	1913	1 10	99
1795	21,179	1040	1 20 $\frac{1}{4}$	49
1796	19,288	3548	1 54	18
1797	17,014	512	1 23 $\frac{1}{2}$	30
1798	18,155	2237	1 8	123
Total 293,350		26,579	1 11	90.9

In this series it appears that the proportion of deaths from Small Pox to the total mortality is 1 in 11, that is, 90.9 in 1000.

TABLE IV.

Years.	Total Mortality.	Mortality from Small Pox.	Proportion.	Proportion to 1000.
1804	17,038	622	1 in 27 $\frac{1}{2}$	36
1805	17,565	1685	1 10 $\frac{1}{2}$	96
1806	18,334	1297	1 14	71
1807	17,938	1158	1 15 $\frac{1}{2}$	65
1808	19,964	1169	1 17 $\frac{1}{4}$	58
1809	16,680	1163	1 14 $\frac{1}{4}$	70
1810	19,893	1198	1 16 $\frac{1}{2}$	60
1811	17,043	751	1 22 $\frac{3}{4}$	44
1812	18,295	1287	1 14 $\frac{1}{4}$	70
1813	17,322	898	1 19 $\frac{1}{4}$	52
1814	19,783	638	1 31	32
1815	19,560	725	1 27	37
1816	20,316	653	1 31 $\frac{1}{4}$	32
1817	19,968	1051	1 19	53
1818	19,705	421	1 47	21
Total 279,404		14,716	1 18.9	53

In this series it appears that the proportion of deaths from Small Pox to the total mortality is 1 in 18.9, that is, 53 in 1000.

DISSERTATION XI.

Narrative of a Hurricane ; with some Reflexions on the Effect of Commotion in the Atmosphere and in the Ocean on the Economy of Nature, and on Life and Health.

THE condition of the atmosphere has, in all ages, been thought to exert an important influence on life and health. This opinion has been still more strengthened by those discoveries of modern science, by which the air we breathe, as well as water, the other great sustainer of life, has been chemically analysed, and its relation to animal nature satisfactorily ascertained. There is no department of natural knowledge in which the energy and success of modern philosophical research has shone more conspicuously than in the explanation that has been given of the constitution of these two elements, particularly in their relation to organic existence. This branch of science may be said to have been created in the course of the last half century, for it is in that time that the nature and relative proportions of oxygen, the great inciter of life, and, together with nitrogen, hydrogen, and carbon, comprising the main constituents, not only of air and water,

but of all animal and vegetable substances. A new world, as it were, has been revealed to the votaries of general science as well as of the medical art; but as this branch of knowledge is now familiar to most persons of liberal education, these topics need not here be further enlarged upon.

But besides the chemical components of the rivers, the oceans, and the atmosphere, there is another aspect of these great features of the globe, of the highest importance in the general economy of nature, as well as to human life. I allude to those wise and wonderful contrivances by which these liquid and elastic fluids are kept in a state of constant motion. Had air and water been permitted to remain in a perpetual, or even long-continued state of stagnation, the whole system of nature, animated and inanimate, would have been deranged, and incapable of subsisting. The putrefactive corruption which would spontaneously take place in a state of rest, in both these elements, would have rendered them unfit for the support of life. The detrimental, nay, the poisonous and pestilential qualities acquired by stagnant air, have been repeatedly and forcibly adverted to in the course of these *Dissertations*.* The like remark applies to large masses of stagnant water.

* See the effect of calms in promoting the plague, vol. i. p. 175, and the noxious effects of want of ventilation, in *Dissertations* III. and VI. *passim*.

Even the Ocean, though provided with so large a portion of antiseptic matter, has been found, as I have been assured by experienced mariners, to become fetid in long-continued calms.

There are few subjects which afford a larger scope of meditation to those who have a taste for investigating final causes, than the present. Besides the action of wind, so well calculated to maintain the salutary condition of those enormous masses of liquid and gaseous bodies with which the terraqueous globe is surrounded, there are other circumstances in nature conducive to the same end. 1st. The surface of the earth has been so moulded and fashioned into inequalities and declivities, as to furnish channels for those perennial currents in the form of rivers, by which the water, supplied by springs and rain, is directed to those great reservoirs from which, by exhalation, the same sluices are again opened for maintaining that perpetual circulation of the aqueous fluid, by which the uniform order of nature is so beautifully and magnificently upheld.

Another important circumstance in the constitution of nature, tending to prevent the stagnation of water and air, is the difference of temperature, as affecting these two elements in their separate or mutual agency; as the rays of the sun in passing through diaphanous bodies, communicate little or no heat to them, unless where refraction is produced by their obliquity, or by

the difference of density, or quality of the bodies through which they are transmitted. The main source of heat from the rays of the sun, is that which is produced by their impinging on opaque bodies on the surface of the earth. It follows from this, that as the air derives its temperature from its contact with the surface of the earth and ocean, it must, in general, be colder than these surfaces. It follows further from these principles, and from those of hydrostaticks, that the water at the surface of a sea or lake, by constantly parting with its heat to the air, becomes of a greater specific gravity, sinks, and is replaced by what is warmer from beneath ; and that conversely, the air having, from the acquisition of heat, become of a less specific gravity, ascends, and is replaced by what is colder from above. It is thus that while a perpetual salutary motion is kept up, the sea, the land, and the atmosphere are preserved in that equability of temperature which is favourable to life, preventing those extremes of heat and cold, which would have rendered every part of the globe uninhabitable.

But it is not in the mere circumstance of temperature that the beneficent provisions of nature are conspicuous : there is a peculiarity in the congelation of water, as distinguished from that of all other fluids, which, in this view, is eminently worthy of notice. It was observed by the Florentine Academy, as long ago as the middle of

the seventeenth century, that when water, in the act of cooling, approaches close to the freezing point, it all at once expands to the same specific gravity which it had at seven degrees above that point, so that when congealed, it is of a smaller specific gravity than in its fluid state. This accounts for ice always forming at the upper surface of water, and not in its general mass: and it is well that Providence has so constituted this part of nature, for otherwise that whole portion of animated beings which inhabit the waters, could not have existed, or their life must have been extinguished. It may be observed, that a like benefit is rendered to vegetable life by snow, without the protection of which, by acting as a blanket, the northern regions of the earth would have been in a state of eternal hideous sterility.

There are further circumstances in the economy of the atmosphere, which exercise an important and conspicuous influence, by affecting the temperature, density, and motion of fluid bodies, and concur with temperature in forming the elements of meteorological phenomena. 1, That constantly increasing tenuity of the air in the upper regions of the atmosphere, whereby its capacity for heat being increased, there is a constant diminution of sensible temperature as we ascend. The effect of this in condensing vapour, is greatly abated by that continued removal of atmospherical pressure, which so much favours evaporation. These opera-

tions go on in the atmosphere to a height which is unascertainable, but not indefinite, as has recently been most ingeniously demonstrated by Dr. Wollaston.* 2, Atmospheric air combined with aqueous vapour, is more rare, and therefore more buoyant than in its simple state. 3, It was first demonstrated forty years ago by Professor Volta of Pavia, one of the most genuine philosophers of this age, and whose name has been so eminently distinguished by subsequent discoveries, that water, in its state of vapour, possesses more electricity than in its fluid state, and therefore carries off a portion of it from those surfaces from which it is exhaled ; and that this surcharge enables the air to hold more of it in solution. Electricity, in this respect, resembles the principle of heat, of which a portion is absorbed in a latent state, in the transition of bodies from a fluid to a gaseous form. It is plain how the unequal distribution of electricity, which thus arises, gives occasion to some of the most striking atmospheric phenomena, by that tendency which electrified bodies have to establish an equilibrium with neighbouring bodies differently impregnated, which they casually approach.

It is further manifest how all these causes bring about, by their variously combined influences, whether in concurrence or counteraction, that salutary order of things which we behold, though

* See Phil. Trans. for 1822.

apparently big with strife and confusion, and occasionally producing great partial calamity, as will appear in the course of this Dissertation.

It would be out of place here, and incompatible with the limits of this work, to indulge in any further illustration of the action and re-action of all these causes, in maintaining that order of nature, which is indispensable to the existence and welfare of animated beings, and in presenting to intelligent beings, that display of beneficence and harmony, of sublimity and beauty, which the more they are contemplated the more they transport the mind. We are indeed, after our most strenuous efforts, compelled to confess that the full comprehension of them, exceeds the utmost reach of our faculties.

I was occasionally led into such trains of thought, in passing through various climates in the course of my publick service, particularly by the occurrence of a Hurricane in the West Indies, in one of the years in which I was Physician to the Fleet on that station.

In the month of August, 1780, after the termination of a very active campaign, Sir George Rodney, the commander in chief by sea, sailed on a cruise with a fleet, consisting of ten ships of the line, and some smaller vessels, rather with a view to the health and discipline of the men, than for the purpose of any warlike operations. It happily occurred to him, that, as there is on that

station, by a sort of tacit truce, a suspension of hostilities in the hurricane months, that is the three months of autumn, much might be gained by avoiding them, and by passing two or three months in a more salubrious climate. Instead of returning into port, therefore, he gave orders to steer for New York. On our return in November, we found that some of the islands had been laid waste by a hurricane in October, and that there had, from the same cause, been great losses at sea.

The first port at which the fleet touched was Carlisle Bay, in Barbadoes, the island which had suffered most by the hurricane. During our stay there, I informed myself of all the circumstances of this recent calamity, partly by my own observation, partly by enquiries of the most intelligent inhabitants, and employed my first leisure, after putting to sea, to reduce them to writing, in the form of a letter, to my highly respected friend, the late Dr. William Hunter of London, whose name will ever be illustrious as a teacher and improver of anatomy, as well as a man of high literary and scientific accomplishments.

Sandwich at sea, off St. Lucia, Dec. 22d, 1780.*

SIR,

Having returned to the West Indies with the fleet from North America, I found that, in

* A ship of ninety guns, and the flag-ship of Admiral Rodney.

our absence, there had been a most violent hurricane at Barbadoes, and some of the neighbouring islands, and I send you the best account of this interesting phenomenon I can collect from the testimony of those who suffered in it, and from the very visible traces of it now under my eye.

It began to blow hard at Barbadoes, on the 9th of October: but it was not apprehended, till next day, that there would be any thing more than such a gale of wind as they experience from time to time in this island at that season. On the evening of the 10th, the wind arose to such a degree of violence, as clearly to amount to what is called a hurricane. At eight o'clock, p. m. it began to make impression on the houses by tearing off the roofs, and overthrowing some of the walls. As the inhabitants had never been accustomed to such a convulsion of nature, they remained till this time in security; but they now began to be in the utmost consternation, and the sufferings and calamities of the people, the ravage and devastation upon every object of nature and art, during the ensuing night, exceed all description. It was thought to be at the greatest height at midnight, and did not abate considerably till eight o'clock next morning. During all this time most of the inhabitants had deserted their houses to avoid being buried in the ruins, and every age, sex, and condition was exposed in the fields to the impetuous wind, incessant torrents of rain,

and the terrors of thunder and lightning. Many were overtaken in the ruins, either by clinging too long to the buildings for shelter, in attempting to save what was valuable, or by unavoidable accidents from the fall of walls, roofs, and furniture, the materials of which were projected to great distances. Even the bodies of men and cattle were lifted from off the ground and carried for several yards. The cries of the helpless, wounded, and dying, could not be heard amidst the crash of ruins and the noise of the elements. At Bridgetown, the dead bodies were too numerous, and the weather too severe, to get them disengaged from the ruins for some days, and they emitted a putrid stench, which affected the air of the whole town. An estimate has been attempted of the number of deaths from returns made to the Governor, and they amounted to more than 3000, though several parishes had not given in their returns when I was there.

All the houses of the island have suffered more or less, and the damage has been the greater, that the buildings were not calculated to withstand such a shock ; for there had been no such event on this island for a hundred and five years. The late hurricanes in our colonies have been confined to Antigua, St. Christopher's, and the other islands in their latitude, and those more to the southward began to flatter themselves with an exemption from such accidents. The large

elegant church of Bridgetown is a heap of ruins, many of the private houses are levelled with the ground, all of them unroofed, and the whole of their carpenter work and furniture destroyed. The greater part of the inhabitants fasted for forty-eight hours, and they were obliged to dig from the ruins the necessaries of life, and clothes to cover themselves. This general wreck had a remarkable effect on the hearts of men. Any one that was more fortunate than his neighbour, prepared victuals to administer to the cravings of the first comer; the owners of warehouses gave what remained of their victuals and clothing almost gratuitously; and ideas of property and interest seemed to be forgotten in the moments of calamity.

All the fruits of the earth then standing have been destroyed, most of the trees on the island have been torn up by the roots, and (what will perhaps give as strong an idea of the force of the wind as any thing) many of them were stript of their bark. The sea rose so high as to destroy the fort, carrying the great guns many yards from the platform, and demolishing the houses near the beach. A ship was driven ashore against one of the buildings of the naval hospital, which by this shock, and the impetuosity of the wind and sea, was entirely destroyed and swept away. As many of the sick seamen were removed as was possible in these circumstances, but three-and-

twenty of them were buried in the ruins. The mole-head was swept away, and ridges of coral rock were thrown up, which still remain above the surface of the water; but the harbour and road have, upon the whole, been improved, having been deepened in some places six feet, in others as many fathom, and the anchoring ground in the road is much better, the crust of coral which had been the growth of ages, having been torn up, leaving a soft oozy bottom. Many shells and fish were thrown ashore, which had been heretofore unknown.

The sufferings and losses by sea have also been great and calamitous. The wind was too violent for any ship to ride it out, and they all pushed to sea, where most of them perished by the mere violence of the weather, without being driven anywhere on a shore. Out of twelve of His Majesty's ships of war that were exposed to it, eight have been totally lost, and out of the crews of them all, not more than ten or twelve persons have been saved.

It has been common to say that hurricanes are attended with earthquakes; but I used to consider it a matter of vulgar fallacy and exaggeration, as might be natural enough in such a general agitation of nature. I have been at pains, from my own observation and enquiry of others, to ascertain this; and I think there can be little doubt that the earth, in such cases, does undergo

a concussion different from what can arise from the mechanical impetus of the wind. I remarked that the flags in the floor of the great church at Bridgetown were set at angles to each other, though they were sheltered from the wind, and nothing had fallen upon them that could produce this effect; casks had changed their position in cellars below ground; masses of rock were moved, which the force of air and water did not seem capable of affecting; and there were chasms in the earth, which indicated clearly some internal agitation. The inhabitants of St. Lucia, as well as Barbadoes, told me they felt it plainly; and, considered as matter of testimony, it is confirmed by this, that at St. Lucia they informed me, that the earthquake happened several hours after the greatest severity of the gale; and people in different parts of the island, who never had conversed together, agreed in their accounts of the time in which it happened; they were people who had experience of earthquakes, and were acquainted with the sensation they produce.*

* I shall here mention some subsequent reflections on this subject, founded chiefly on the late improvements in chemical philosophy, particularly the very important discoveries of Sir Humphry Davy, which throw great light on the general system of nature, and accord with some of the theoretical ideas thrown out in this letter. Our globe may be conceived to have arrived at its present state, through the following series of events:—Let us suppose that two planets, or asteroids, to have been originally created, the one consisting of the metallic bases

This hurricane has principally affected the middle Carribee Islands, and chiefly Barbadoes,

of the several species of matter now existing, the other of oxygene combined with azote and hydrogen, in the forms of water and atmospherick air. Let us further suppose, that at some remote point of time in the abyss of antecedent eternity, these two bodies, in their movements through space, encountered each other. The consequence would be a mighty conflagration in the act of combination, as in common combustion ; or like what may be conceived to take place in an *aërolithe** on entering our atmosphere ; for from the circumstance of these being always in an ignited state when they fall, it is evident that they are strangers there, and that they consist of metallic bases, which catch fire on the contact of oxygene. It is manifest, that all the bodies forming the exterior part of the earth, would, during this conflagration, assume the form they now have ; but they would form such a crust as would prevent the contact of the oxygene with the more deep-seated materials, so that the combustion would cease before all the metallic bases were neutralized. This would leave the earth in the form of a sphere, surrounded with one immense ocean. But as the deep-seated metallic bases would be subject, from time to time, to the casual irruptions of water, the gas thereby generated would heave portions of the crust above the surface of the water, producing the dry land, and all its inequalities. Water would,

* Otherwise called meteoric stone, to which it is evident the presence of oxygene is new, and therefore could not have been generated in our atmosphere, but must either be a small portion of matter wandering through space, or a fragment launched by centrifugal violence or other accident, from asteroids circulating round the earth, too small to be visible. Should one of these, whether metallic or oxygenous, visit our ball, bodily instead of representatively in some future age, as I have alleged to have happened in some past age, it probably would not be very welcome, as it could hardly fail to induce some revolutionary changes in the present order of things.

which lies in latitude 13° , and has not experienced the like for more than a century ; whereas

from time to time, insinuate itself so as occasionally to reach these bases, and produce volcanoes and earthquakes, such as have occurred in the ordinary course of nature in past times, by the report of history, and from the observation of our own times, from the extrication of gases irresistibly expansive and explosive. And is it not possible, that under that singular impetuosity of the torrents which takes place in a hurricane, the water may force its way by chasms and caverns to some of those metallic bases which had never before been brought into contact with water ?

These may perhaps justly be deemed wild speculations, mere crude conjectures, but they are nevertheless in accordance with those principles of chemical philosophy, which have been developed by the discoveries of the present President of the Royal Society, and must constitute the main elements in any future and more sound theory of the earth, than that which has been sketched above : nor can they fail in time, from the range which they afford for diving into the depths, and soaring to the heights of our terraqueous globe and its atmosphere, to give birth to further important discoveries and sublime meditations on the order and administration of the planet we inhabit.

The connexion of atmospherical with subterraneous commotion, has in no instance, that I know of, been more strikingly proved, than in an account published in the *Journal de Physique* of 1821, and in the *Edinburgh Journal of Science* for 1822, of the circumstances attending a hurricane and earthquake, which occurred simultaneously in the island of Zante, on the nights of the 29th and 30th of December, 1820. A luminous meteor appeared a few minutes before the first shock of the earthquake ; then a tremendous storm of wind attended with thunder and lightning, loud, vivid, and long continued ; torrents of

the islands between latitude 17° and the tropic, are seldom exempt from them for more than seven or eight years together. The islands that have suffered most on this occasion, besides Barbadoes, have been Martinique, St. Lucia, and St. Vincent. They had it in a small degree at Antigua, St. Christopher, the Virgin Islands, and also at Grenada. At Tobago there was rough weather at the time, which did no material damage.

This is the extent of it north and south. It would be curious to ascertain how far it extended east and west, but of this we have only imperfect accounts. A ship that arrived at Barbadoes six days after, had a gale of wind about the time of the hurricane, which was remarkable only for this, that the wind blew all round the compass, a

rain, with hail-stones, weighing from six ounces to a pound and upwards. After the last shock of the earthquake another meteor appeared. The thermometer fell from 65° to 25° . We have no account of the barometer.—In this description it is mentioned that the hurricane was at its utmost pitch of violence in the night time on both occasions; and it is somewhat remarkable, that though in all other hurricanes, as well as common storms, which I have either observed, heard of, or read of, the like has happened, yet it has no where, that I know of, been the subject of remark, far less accounted for: nor do I feel myself prepared to discuss this question, but hope that this, as well as some other unsolved problems in the science of meteorology, particularly those relating to barometrical indications, will be undertaken by some one more competent to the task.

circumstance which distinguishes the hurricane from all other gales within the tropics ; the course of nature being so far inverted, that the direction of the trade winds, at other times constant and invariable, is not then observed. It is true that at Barbadoes, the greater part of it was from the N. E. ; but an hour or two after midnight it was for a little time due W. and was more or less in all the intermediate points. It was very irregular at different places in this respect, for at St. Lucia they had it not at any time from the west, but the ships that were driven from that island had it from all points. The progress of it westward was slower than might be expected, for at St. Vincent and St. Lucia, which are from eighteen to twenty leagues from Barbadoes, and to leeward of it, the hurricane was thirteen or fourteen hours later, and not near so violent. They had it at St. Domingo in a still less degree on the 13th and 14th of October. It was also remarked, that the ships which put before the wind were not carried with the velocity that might have been expected from its violence. Those who outlived the storm till the morning were surprised to find themselves so near the spot from whence they had been driven the preceding evening.

The influence of this general tumult of nature upon the health of man is none of the least curious of its effects. I have made much enquiry upon this head, not only at the medical Superin-

tendants of the Naval and Military Hospitals, and the physicians of the place, but at private persons; and I find, that so far from its having been productive of sickness, there has been less of it since, and even that most of those who laboured under disease at the time benefited by it, except the very old and delicate, who suffered from mechanical violence, or the subsequent want of shelter. This is a fact so paradoxical, that if I had not a concurrence of testimony, and in some degree my own observation, I could neither credit, nor would venture to relate it. It had a visibly good effect on the diseases of the country; fevers, fluxes, and chronic diarrhæas the consequence of dysenteries, were also cured by it. But the diseases upon which it operated most visibly and sensibly were pulmonic complaints. Some cases, supposed to be beginning consumptions, and even the acute state of pleurisy, were cured by it. In the more advanced and incurable state, the hectic fever was in a great measure removed, and a temporary alleviation at least procured. A delicate lady, of my acquaintance, was ill of a pleurisy at the time, and passed more than ten hours in the open air, sitting generally in a plash of water from the rain that fell; she had no more of her complaint, nor any return of it; and I saw her a few weeks after, in better looks and general health than she had enjoyed for a great while before. It was a general obser-

vation, that people had remarkably keen appetites for several days after, and a number of those whom I knew, formerly thin and sallow, looked fresh and plump a few weeks after, though the unhealthy rainy season was then hardly over.* I believe it will be difficult to account for this on any known principles, though we may advance some of those vague analogies and conjectures of which a great part of all medical reasoning consists. I should have observed before, that the air was excessively cold during the violence of the hurricane; and, as vicissitudes in point of cold and moisture are extremely productive of complaint in this climate, one would suppose *a fortiori* that this would have had a detrimental effect; but the animal economy is anomalous and full of enigmas. It is probable that the agitated state of the mind had some effect in averting the natural effects of the weather; just as a person when under exercise, or having his attention taken up on some interesting object, is less apt to suffer from exposure than if the body or mind were unoccupied. We can conceive too, that there may be something in it analogous to sea sickness, under which the body not only receives no impressions

* It has been said before, that there is probably some connection between earthquakes and hurricanes, and it is mentioned in the Journal of the Royal Institution for 1817, that intermittent fevers were cured by an earthquake at the Carraccas, in March 1812.

of disease, but the general health is sometimes mended. And may not some benefit, in the one case as well as the other, arise from the suspension of the functions of the stomach, and the other assimilating organs, which the body endures in both cases without the same detriment that it would, from a fast in what may be called cold blood? Neither is it ridiculous to suppose, that the purity and coolness of the air would have a happy effect on the animal frame, especially as the diseases of the lungs were most benefited by it. Dr. Priestley's doctrine of the exhalations of organic bodies in polluting the air, or of agitation in purifying it, is well known. I shall presently venture a conjecture, that almost the whole phenomena may have arisen from the act of establishing the balance of electricity, which in its passage from the air to the earth, or from the earth to the air, may be conceived to convey a salutary stimulus to the body.

I shall conclude this letter, already too long, by hazarding a few conjectures and reasonings concerning the nature and cause of Hurricanes. Some principles have been discovered by modern philosophy which go a certain way in explaining the economy of the atmosphere. First, In the first place, it is ascertained by experiment, that the rays of the sun are not essentially hot, but produce heat by their action on opaque bodies, or by repeated refraction in passing through pel-

lucid bodies of different specific gravities. The temperature of the atmosphere, therefore, depends on the contact of the air with the earth and water of the globe, and is not affected by the passage of the sun beams (or very inconsiderably from the refraction caused by its different density at different heights), but its heat and cold are according to the aggregation and colour of the subjacent parts, and the obliquity of the rays. Secondly, It has been discovered that air, in a true and proper sense, unites with and dissolves water, forming with it a homogeneous elastic fluid, of less specific gravity than pure air. Thirdly, Evaporation of water is found to be in the compound proportion of the heat it undergoes, the dryness of the air in contact with it, the superincumbent pressure it endures, and, it may be added, the electricity it contains; for the more water is impregnated with electricity, the more volatile it is, that is, the more miscible it is with water and the more convertible into vapour. Fourthly, All fluid bodies, when allowed to act upon each other in one mass, either rise, sink, or press laterally, according to their density or position. This difference of density in the air is commonly supposed to give occasion to all those motions in which wind consists.

The density of the air is affected by the height and pressure, by its heat or cold, by the quantity of water it holds in solution, and by the electricity it contains.

Constant and simple causes necessarily produce uniform and certain effects; complex and variable causes produce contingent and undeterminable effects. If the whole of the external globe had been either water or an even surface of earth, and if at the same time the axis of the globe had been at right angles to the ecliptic, thereby destroying the distinction of seasons, there would have been a constant or trade wind at all times in all places. But as things are, there are a number of causes which check and counteract each other, such as the difference of seasons, the diversity of sea and land, the different elevation and extent of land, and even the difference of vegetation and culture on its surface. In those places where the causes are most uniform we may expect the greatest uniformity of effect; and accordingly, where there is a great extent of the same continued surface, as in oceans, and at the same time a small difference in the obliquity of rays and the vicissitude of seasons, as between the tropics, there we find the wind to blow nearly the same way at all times. But, in consequence of this very uniformity, the causes of motion become more accumulated than in the variable regions where the hot and cold, the dense and rare, and the different impregnations of electricity in the several stages and tracts of the atmosphere, by being more frequently jumbled together, are from time to time brought to a balance and produce but moderate effects. The air in all regions of the

earth is, we know, excessively cold at certain heights of the atmosphere, and the mountains of Peru are as cold as those of Lapland. In consequence of the long and uniform blowing of the trade winds, the cold air at top, as Dr. Franklin observes, comes to be poised upon rarer and warmer below, and there needs only some minute circumstance to destroy this equilibrium, and make the cold air rush down to the place it would naturally occupy by the laws of hydrostaticks. This circumstance is probably the recess of the sun; in consequence of which, in the month of August, the cold winds rush from the north-east (the quarter from which hurricanes begin, and gales and heavy rains generally come), and from the upper regions of the atmosphere, and the seasons in the West Indies continue rainy from thence till the winter solstice. From the slow progress of a hurricane, having been twelve hours travelling eighteen leagues from Barbadoes to St. Vincent, from the local nature of its effects, from the winds blowing from different points at the same time in places near each other, from great degree of cold which could proceed only from the air of the upper regions, it would appear, I say, from all these circumstances, that the wind, on these occasions, blows rather in a vertical than a horizontal direction.

Many circumstances concur in shewing the very partial and local effects of land in agitating

the atmosphere, and this is no where more remarkable than in the West Indies. The Island of Barbadoes, for instance, has suffered from droughts the last eighteen years, while in St. Vincent, a few leagues directly to leeward of it, there was no such complaint. Antigua is exactly to St. Christopher's what Barbadoes is to St. Vincent. There is always more rain on shore than at sea, on the mountains than on the plains, on woody ground than on what is cleared. Hurricanes, as we have seen, are also extremely partial, when the body of the atmosphere comes from the ocean, prepared for agitation and explosion from the varieties of its portions in point of density, temperature, and electricity; if it meets with a continent, the effect is divided and diffusive; but meeting with islands, particularly if they are mountainous, it discharges itself in a *focus*.*

* Perhaps the great comparative infrequency of hurricanes at Barbadoes, none having happened there till now, since the year 1675, may be in part accounted for by its superior flatness in comparison of the other islands, which are all mountainous. Its geological structure is indeed strikingly different from the others, and accounts for its being less elevated. It is almost entirely composed of calcareous masses of shell, corallines, and madrepore, and without those vestiges of igneous eruptions observable in the other islands, in most of all of which there are craters or other traces of ancient volcanoes, some of which are still in action. The Carribee Islands stretch in a sort of bow, from the southern point of North America to the mouth of the Orinoco in South America, and have evidently been

The most northerly islands of the Carribees are most subject to this concussion, probably from

raised by subterraneous fire operating in the whole of that line ; and the sea is so shallow in the intervals of these islands, that the bottom can be seen from a ship. The island of Barbadoes lies about twenty leagues to windward of this line, where it would appear that the expansive force of the gas has acted with less force, so as merely to heave the immense strata of the above mentioned organic remains above the surface of the ocean, and there leave them without coming to a volcanic explosion.* The great *strata*, or cakes of calcareous matter, consisting of accumulated testaceous and zoophytick bodies generated at the bottom of the sea, bear manifest marks of this operation, for they are forced from their horizontal position in broken masses, forming in some places long caves, where they stand at angles to each other. Almost all the islands in Polynesia, and many in other parts of the globe, have had a similar origin. When this is taken into consideration, and also the prodigious quantities of the like organic remains, in marble, limestone, and chalk, a curious question arises, whether all this mass of matter, which bears a sensible proportion to the whole solid part of the globe, could have been formed by the organs of living animals, or from calcareous matter taken in by them in their food. It is so difficult to comprehend how and where these animals could meet with so much calcareous earth, that the supposition of its being elaborated by their organs, seems the most probable hypothesis. If this can be fully demonstrated,

* All human art and skill consist in the imitation of the operations of nature ; and in no instance is this mimicry so conspicuously displayed as in the present instance ; for is it not the same extrication and expansion of elastic fluids, that by their stupendous force, lift mountains and islands, nay, continents, from the bottom of the ocean, which the puny hand of man employs in his most boasted agencies ? witness gunpowder, and the steam engine !

the vicinity of the cold winds of the temperate zone, which rush in upon the recess of the sun,

it will imply that some of the metallic bases, such as calcium, is not a simple substance, as has been thought, but resolvable into those principles which compose the aliment of animals. A celebrated and ingenious chemical philosopher, Mr. Hatchett, mentioned to me a fact, very much in favour of this. He remarked, that a chick, after the completion of incubation, possessed such a quantity of calcareous matter in its bones as could not easily be accounted for, but on the supposition of its being created in the course of the animal's growth from the materials of the yolk and albumen, all connexion with the shell being apparently cut off. This very nice question, which though belonging to minute objects, bears a most curious and important relation, not only to Chemistry and Physiology,* but to the formation and distribution of the materials which compose the globe of the earth.

Just as this note had been prepared for the press, Dr. Prout, so well known by his accurate and useful researches on Animal Chemistry, obligingly favoured me with a very elaborate and highly interesting tract on this subject, consisting of "Experiments on the changes which take place in the fixed principles of the egg during incubation," which will be found in the volume of the Philosophical Transactions, now in the press. This Gentleman, with that philosophical caution and reserve, which belongs to his character, does not give a decided opinion on the subject, alleging, that though there seems to be insuperable difficulties to the opinion of the lime in the bones being derived from the albumen, yolk, or shell of the egg, "he will not be bold enough to assert, in the present state of our knowledge, however strongly he may be inclined to believe it, that,

* See Elements of Medical Logick, page 58, 2d Edit. London, 1821.

and the tract of islands lying in a chain from east to west, between the 17° of latitude and the tropic, second each others effort, in attracting the electricity and oversetting the balance of the atmosphere. The effects of high land in condensing vapour, especially in the torrid zone, seems to depend on an electrical principle. In the cold and temperate regions, rain seems more owing to the mixture of cold and warm air, and more to the communication of electricity within the tropics, for there is more thunder and lightning here, and the common rain resembles that of the thunder-gusts of the north. I should have taken notice that the hurricane was attended with tremendous lightning. They told me at Barbadoes that it was in large steady blazes; and not much of the forked and darting kind. A physician who had been in the north of Europe, said, that in the north-east there was the Aurora Borealis, an appearance not natural to this climate. The French inhabitants of St. Lucia said there was a great deal of what they call the *Feu de St. Helme*, which I believe is the *ignis fatuus*; and an inhabitant of St. Vincent said, the lightning falling upon the earth, spread around like the sparks from an anvil. Electricity has a natural tendency to run off at points; and it is probably owing to this, that a shower, even at a distance, seems to descend in within certain limits, this power is to be ranked among the capabilities of the vital energies."

columns. In crossing the tropic, in our passage from North America, I was lucky enough to see a water spout, which advanced in the van of a shower and squall. It was a cylinder, with a defined and sharp outline, plainly directed downwards, for it descended in an angle to the sea, with the acute angle to windward. There was no mark of a whirlwind; nor was it a solid body of water, but an excessive heavy rain; and may be considered as the portion of a cloud, or body of vapour, which, by drooping lower than the rest, and being a little detached, had its electricity drawn off, so as to be condensed in a more violent and partial manner.

I shall now conclude this letter, as I am sensible, I am tiring you, though I have not exhausted the subject, and I am running into disquisitions more curious than useful. I am sorry I have met with nobody that made any observations on the barometer* or thermometer during the pheno-

* A few months after this letter was written, I was informed by Dr. Warner, a very respectable physician of Antigua, that on occasion of a hurricane in that island some years before, he took refuge in a cellar, and having carried a barometer with him, he observed that the quicksilver fell to $27\frac{2}{8}$. This point of meteorology is further illustrated by a singular crisis of the weather which occurred over the greater part of Europe, in the month of December 1821. The whole of that month was uncommonly tempestuous, but the incidents alluded to happened on the 24th and 25th days of it. These consisted in violent storms, such heavy rains as to cause calamitous inundations,

menon I have attempted to describe. I have never known the thermometer lower than 75° , nor

thunder and lightning of unexampled intensity and duration ; and a fall of the barometer below what is to be met with in the records of that instrument. In London and at Cambridge it fell to 28 inches, in the neighbourhood of London it fell to 27.98, in Cheshire to 27.3, at Lyons to 25.9, at Augsburg to 25.6. The like accounts were received from other parts of Europe,* and every where attended with storms, long continued peals of thunder, and vivid lightning ; igneous meteors were seen at Bamberg and Frankfort. At Vienne in Dauphiny it is described as if the whole heavens were on fire. It is further remarkable that the winter was singularly mild all over Europe, so much so in Russia, that great distress arose from the ground not being sufficiently hardened to bear sledges, the only means of conveyance in ordinary winters ; nor was the cold sufficient to freeze their fresh animal food in the customary manner of preserving it in that season without salt. The only connexion of these atmospherical phenomena with subterraneous disturbance was, that a volcano broke out at this time, in a district of Iceland, where none had occurred since the year 1612, and an earthquake was felt at Mentz on the 25th. It is remarkable, that the barometer fell to 28.08 in London,† during the earthquakes of Calabria in 1783.—I shall conclude this digression with the mention of one other meteorological incident, which occurred in the West Indies since this letter was written. In the year 1812, there was an eruption of a volcano in St. Vincent ; and it was found that an earthy dust had been ejected from it into the upper regions of the atmosphere,

* See more particulars of these phenomena in the London and Edinburgh Quarterly Journals of Science, and the Bibliothèque Universelle of Geneva for 1822.

† See Phil. Trans. for this year.

higher than 85° at sea in this climate, and the range of variation in the barometer is much less than in Europe.

I am, dear Sir,
with unfeigned esteem and regard,
your most obedient, and most humble servant,
(Signed) GILBERT BLANE.

P. S. It is a general traditional observation in the islands, that the hurricanes fall within two or three days of the new or full moon. This happened in the case I have related ; but an ingenious member of the Royal Society* has lately shewn by reasoning, clearly deduced from observation and calculation, that there is no foundation for the supposed influence of the moon upon the weather. This is satisfactory enough with regard to England and the variable climates, but I am

where it must have met with a current of westerly wind, for the Island of Barbadoes, twenty leagues to the eastward, was covered with it more than an inch thick, and it fell in large quantities on the decks of ships, more than a hundred miles to windward. It would appear, that the portion of the atmosphere which is carried to the westward, forming the trade winds, is returned in the contrary direction in an upper current ; performing vertically to the atmosphere, what the gulf stream in those seas does to the ocean horizontally.

* See an article in the Phil. Trans. for the year 1766, by Dr. Horsley.

inclined to think, from what I have observed, that within the tropics the moon does affect the weather, and it is what might be expected from the greater uniformity of the wind and weather.

To Dr. Hunter.

DISSERTATION XII.

*On the Effect of Mechanical Compression of the Head, as a Preventive and Cure in certain Cases of Hydrocephalus.**

IT was the Author's original intention to have included in this work a much larger extent of matter, but as the preceding Dissertations have run out to an unexpected length,—as some consideration is due to the time and patience of readers (though it has been his most anxious study to express what he has said in as few words as possible) in this age of unexampled and exuberant fertility of the press on all subjects,—under the uncertainty, also, of the publick being disposed to concede the same importance to productions as the partiality natural to authors ascribes to them, he is induced to withhold many minor tracts and observations which he finds in his notes, or dispersed in various collections, and will satisfy himself with recording one of the latest that has occurred in the course of his practice.

* This was originally published in a Periodical Journal, in 1821.

In reflecting on the circumstances which characterize the history and description of hydrocephalus, some of the chief of which are, that it is very seldom met with but in very early life, and most commonly in infancy before the bregma is closed,—that there is in most cases a preternatural size of the head,—and that it is usually attended with a rachitic state of the bones and a general scrofulous flaccidity of the soft parts, and runs in particular families,—it occurred to me that the distension of the head and bregma is owing to a want of firmness and due resistance in the bony *compages* of the skull, which consequently yields to that effort of pressure with which the brain in its growth acts on its parietes. In reasoning further on the subject, it appeared to me conformable to some of the most approved principles of physiology, that, as there is a certain degree of tension and pressure necessary to the sound condition and action of parts,* the withdrawing of this, by inviting afflux and congestion, produces serious effusion; and that for the like reason, there may be a deficiency of that interstitial absorption upon which the healthy state of this and all other soft parts of the living frame depends.

It was reflections of this kind, whether well or ill-founded, which some time ago suggested to

* See this subject further illustrated in *Elements of Medical Logick*, page 95, et seq.

me that mechanical compression of the head might be of use in the cure of hydrocephalus, and which induced me to make trial of it in a case which occurred to me last year, (1820). A child, aged thirteen months, had a head of a preternatural size almost from birth, and the bregma was preternaturally large. The conformation of the child was otherwise defective; for there was a visible curvature of the spine, indicating a rachitic diathesis. He had for several months been subject to drowsiness; and latterly it was evident, from his screaming, and from raising his hand to his head, that there were occasional paroxysms of head-ache. There had also been for some time past a dilatation of the pupils. The functions of the bowels were not so much disordered as is generally the case in this disease, which was still in an unformed state.

I directed the head to be swathed with a roller, as tight as could be done without producing pain or uneasiness. The only other remedies were three leeches, applied once only to the temples and forehead, and a purge every two days of rhubarb and sulphate of potash: mercury was not used in any form. An immediate amendment took place, and continued, so that all complaint was removed in less than three months, except the curvature of the spine; and he has continued well till now, that is, for eighteen months.

The only other case which has since occurred

in my practice has been one which did not offer so fair a field for the trial. The subject was a child of three years of age, with a head preternaturally large, and the bregma not yet closed. No symptoms of hydrocephalus had appeared, but only a state of general delicacy. The swathing has apparently been of benefit, and may probably prevent a disorder naturally to be expected from such a conformation.

Before sitting down to write this, thinking it likely that a view of the subject, seemingly so simple and obvious, might have occurred to others, and deeming it possible that similar trials might have been made, and the practice exploded from being found unsuccessful, I consulted all the works I could meet with, but without finding any intimation of the theory or treatment here described. I therefore felt it my duty, even at this early period of my experience, to suggest the trial of it to others. I have elsewhere remarked, (*Medico-Chir. Trans.*) that one of the great purposes of the general and early diffusion of the valuable information contained in periodical works, is to offer suggestions of this nature, in order that they may undergo either a speedy confirmation or refutation, the experience of one individual being too limited to afford that satisfactory induction which medical truth demands, or too protracted for the decision of questions which, for the good of mankind, ought to be as prompt as possible.

It is necessary to caution practitioners against a too active use of this method in recent and acute cases, where there is inflammatory affection, and therefore such a degree of tenderness as may make compression painful and prejudicial. Let it be understood, that the practice here recommended is meant to apply chiefly to those cases in which there are indications threatening an attack of this disorder, such as a large head, a tardy closing of the bregma, dilated pupils and rachitic diathesis, or to those in which the acute stage has been vanquished by the usual remedies, and when there is a protracted or imperfect cure, or the apprehension of a fatal result from relapse. The times, circumstances, and degrees, in which this practice is admissible, must be left to the judgment of practitioners.

I would by no means rest the expediency of making further trials of this on the theoretical views which suggested it, but wish to refer entirely to experience. I am aware, indeed, that the theoretical views of others, being adverse to those which I have advanced, may produce a reluctance on the part of some practitioners to pay attention to what is here recommended. This disease has been referred by some to a depraved state of digestion, indicated by a distended abdomen and a vitiated state of the feculent matter, caused by depraved action of the intestines, mesentery, and other chylopoietic viscera. But such disorders occur in innumerable in-

stances without producing hydrocephalus; though it is easily conceivable how this state of bowels, which is so likely to arise in subjects predisposed to this disease, may excite or exasperate it, as well as all others of a rachitic or scrofulous nature, by producing depraved assimilation and scanty nutrition.

Other theorists are fond of representing hydrocephalus as proceeding, in most instances, from an inflammatory affection of the brain. There can be no doubt of such affection frequently taking place here; but I believe there are few instances of the acute state of hydrocephalus coming on without a predisposition consisting in an enlarged head and bregma: and it has been plausibly maintained by some, that a relaxed state of vessels, in particular parts, is the main cause of all inflammation. At all events, as it is found to run in families, this is a clear proof that it is a disease proceeding from a particular constitution, and not always depending on accidental causes. One of the cases here related, having manifest symptoms of predisposition, which were successfully combated, strongly proves and illustrates this, and affords a useful practical hint to parents and medical attendants. I knew a family of the highest rank and respectability, who lost the whole of their male offspring by this disease, and died some years ago without heirs of their own body to their titles and

estates. Some of these children would probably have been saved, had the preventive measures here stated been then known.

But, leaving all theoretical discussions out of the question, it is the Author's purpose merely to submit what he has said as a suggestion to others who may be induced to make trial of it, and report the result to the publick.

He has only further to suggest, that in case the utility of this practice should be confirmed by further experience, it ought to be resorted to as a *preventive* as well as *curative* treatment, and applied whenever infants are perceived to have the head and bregma preternaturally large, without waiting its alarming manifestation by symptoms of hydrocephalus,—symptoms which, by this measure, may be happily averted.

Since the preceding article was written, several cases have occurred to some of my friends, as well as myself, strongly in favour of the practice here recommended; and two highly respectable and important testimonies of its value have been communicated by gentlemen, who are strangers to me, which I shall here give at length. One is from the late Dr. Girdlestone, an eminent practitioner of Great Yarmouth, in a letter to the Editor of the Medical and Physical Journal, enclosing the following statement from Mr. Costerton, an intelligent surgeon of the same place.

“ Mary Monks, a married woman, was delivered of a male child on the 3d of October, 1819. About a fortnight afterwards, the child became ill with sickness and fever, and a constant moaning and crying, without any very apparent dilatation of the pupils of the eye, but a considerable projection of the left parietal bone. The child visibly wasted; small doses of calomel were administered; but as no benefit was obtained by pursuing these means, and the parents were poor, the medicines were discontinued, and after wasting for two months the child died. There was no examination of the head, as I was not apprised of the child's death till after he was buried.”

“ On the 9th of April, 1821, this woman was brought to bed of another boy, born apparently in good health. About three months after, his head became enlarged in the same manner as the child who had died. In the beginning of last October, the mother took him to Dr. Girdlestone; and he learning that the same appearance had been discovered in the other child before its death, and that I had delivered her of each of these children, deferred recommending any medical treatment, until he had some conversation with me. On Dr. Girdlestone seeing me, he detailed to me a paper he had just read in the London Medical Journal of October, from Sir Gilbert Blane, and gave it as his opinion, that this child was a proper subject for trying the treat-

ment recommended by that physician. This child has the same symptoms of wasting, &c. as the other who died, and, superadded to them, a rupture, on each side of the body. Dr. Girdlestone wished me to try first, without the aid of medicine, the simple effect of pressure on the brain; and as I could not succeed in confining a bandage round the parts, I put a double strap of adhesive plaister round the head, which completely answered the purpose. The straps remained firm as they were originally fixed. The child gradually improved under the pressure; and the head, which was originally bald, began to be covered with hair, and to acquire more uniformity; and as the muscular strength of the abdomen encreased, the ruptures disappeared. The teeth are cutting, the child is growing stronger, and may be said to be nearly well.

(Signed) CHARLES COSTERTON."

Yarmouth, January 29, 1822.

The other testimony is from Dr. Thackray, of Cambridge, whose learning and practical eminence, will have their due weight on the reader in appreciating his evidence. In a letter to the Author, dated Cambridge, the 10th of September, 1820, he says, "I am indebted to you for the idea of bandaging the heads of children, where that organ is larger than usual, and when the sutures do not seem ready to close in the usual

time. In two instances I have seen great benefit from it. In one, where there seemed every proof of water being accumulated in the ventricles, the bandage seems to have effected a complete cure. The child, (thirteen months old) had never been able to sit up, and was easy only in the horizontal posture, was seized with fever, screamings, and most horrid squintings, and very spinage-like stools. Leeches often repeated, and calomel in large doses, reduced the fever, but the squinting and inability to sit up continued. I now tried a bandage of adhesive plaister around the head, and almost instantly the *strabismus* left the child, and she has gradually progressed to a firmness of muscles, can now sit in the nurse's arms, and can bear to be danced about. She still wears the bandage, which has now been on above six weeks. I conceive the child owes its life to this practice."

The substituting straps of plaister for bandages of cloth, I consider as a judicious improvement on my method. I have only to add, that this practice will, perhaps in the eyes of some, be more interesting from its being one of the few practical truths that have been discovered on suggestions derived by physiological theory.

[In the year 1829, Sir Gilbert Blane submitted to the Board of Admiralty the Proposal described in the following Plan and Regulation, and having been approved and formally authorized by that Board, it was accordingly carried into execution, and the first Adjudication of the Medals has taken place, as will appear by the subsequent Narrative.]

PLAN and REGULATIONS of the Establishment, and Adjudication of Two Prize Medals for the Encouragement of the Medical Officers of the Royal Navy, and the Improvement of Physic and Surgery in that Department of the Public Service. Founded by Sir GILBERT BLANE, Bart. First Physician to the King, F. R. S. S. Lond. and Edin. Gott. Member of the Imperial Academy of Sciences of Russia, of the Institute of France, &c.

1. The Founder, considering how much it will conduce to the advancement of the public service, that emulation should be excited among the Medical Officers of the Royal Navy by honorary distinctions for professional merit, has vested the sum of three hundred pounds in the three per cent. consolidated Bank annuities in the Corporation of the Royal College of Surgeons of London in trust, with the dividends which shall be from time to time receivable, for the purpose of conferring, once in two years two Gold Medals, of equal value, on two medical officers, surgeons of ships of war, in commission, or assistant surgeons of King's ships in commission, not bearing sur-

geons, who, in the time required, shall have delivered, into the proper office, Journals, evincing the most distinguished proofs of skill, diligence, humanity, and learning in the exercise of their professional duties : these journals to be delivered in the form in which they have been kept from day to day, stating the symptoms as they shall have occurred at the time ; but without prejudice or hindrance, to their making such observations, practical or theoretical, as they may judge proper to annex to them.

2. The first selection to be made by the Medical Commissioners on the 12th of August, 1831, from the journals delivered between the 12th of July 1827, and the 12th of July 1831. All future selections to be made on the 12th of August, at the interval of two years from each other, from the journals delivered in the two preceding years up to the 12th of July immediately preceding such selection.

3. In the selection of these journals the Founder proposes that the Medical Commissioners of the Navy shall, out of the whole journals delivered to them in the course of the intervals above specified, make choice of such as in their judgment possess the highest degree of merit, in number not more than ten, nor less than five, which shall be transmitted to the Founder during his life-time,

for his selection out of the number so sent, of two, or one, in case there should not be another of sufficient merit, the author or authors of which, in his judgment, may be most deserving of the prizes. And after his decease, the said journals to be conveyed to the President of the College of Physicians, who, after due examination, is to communicate to the President of the College of Surgeons, and, after proper deliberation, the said presidents are to call to their assistance the senior Medical Commissioner of the Royal Navy, and jointly with him select from the said journals one, or two, the author or authors of which, in the opinion of the majority, possess the highest merit and become thereby entitled to the medal or medals. The medal or medals, when adjudged, are to be put into the hands of the attending Medical Commissioner, to be by him presented to the successful candidate or candidatess. All the journals of the first selection to be returned into the custody of the Medical Commissioners.

4. In case of the impossibility of performing the before-mentioned duties through the illness or unavoidable absence of the parties described, the duty is to devolve on the next in rank, that is, on the senior censor of the College of Physicians, the vice-president of the College of Surgeons, or the junior Medical Commissioner.

5. In case it should happen at any of the periods of adjudication, that in the opinion of the Founder, or of the two Presidents, after his decease, there shall not be found a journal or journals of adequate merit to entitle any candidate to the prize, the medal or medals shall be withheld until the next period of adjudication, and the unadjudicated medals are to be conferred on such as may possess sufficient merit over and above those subject to adjudication at that period. But this regulation is to be construed and limited, that no more than four prizes shall be adjudicated at any one period: and if the unadjudged medals should exceed this number, their value in money is to be given to the supplemental fund for the children of Medical Officers.

6. In case at any time the Founder, or the two Presidents shall omit to make the adjudication for a longer period than three months, they shall be considered as having forfeited their right, and the ultimate selection shall devolve on the Medical Commissioners, who in case of difference of opinion may call in such a referee as they may judge necessary or advisable.

7. The Founder shall provide and deposit with the Royal College of Surgeons, the die engraved for the medal, from which they will cause the medals to be struck at the prescribed periods,

and to be delivered to the Medical Commissioners to be presented to the successful candidates.

8. No successful candidate to be admitted as a competitor a second time.

9. The Presidents of the Royal Colleges of Physicians and Surgeons, and the senior Medical Commissioner, to be considered as guardians of the fund and its equitable administration.

10. In case any of those surgeons whose journals have been given in, should be paid off previous to adjudication, or they should have been appointed to an hospital, or any other situation on shore, except that of Medical Commissioner, such surgeons shall be deemed eligible candidates for the medals in case of adequate merit.

11. After a lapse of not less than ten years from the decease of the Founder it shall be competent for the Presidents of the two Royal Colleges, and the Medical Commissioners of the Navy, to hold an interview for the purpose of consulting whether any and what additions or alterations would be advisable in the preceding plan and regulations, and to adopt them, in case of their being unanimous for the adoption : subject nevertheless to the approbation of the Lord High Admiral, or the Commissioners for executing the office of the Lord High Admiral.

Recommendations and Suggestions.

The Founder, with all deference to the high professional authorities who are to adjudge the medals, begs to suggest and recommend as follows—

1. That a book be kept in the custody of the Medical Commissioners of the Royal Navy, wherein is to be transcribed the Plan and Regulations, and to serve also as a Record of the periodical adjudications, and wherein not only the names of the successful candidates may be inscribed, but also of all those of the first selection; among whom it cannot be doubted that there will be found tokens of merit which may go without their due reward from the limited number of medals, and all of whom will of course possess a considerable share of merit above the unselected, and be deserving of consideration.

2. That there be transcribed into this book of record such remarks as may have arisen out of the examinations, deliberations, and discussions, of those appointed to adjudge the medals, and which may prove a source of much valuable information, not only for the interests of the navy, but of the community at large, while it will open a source of liberal and useful intercourse between the members of the different public professional institutions of the Empire, provided some degree of publicity should be given to them.

DISSERTATION XIII.

Comment on the Journals of the Naval Surgeons who are the Competitors for the Prize Medals founded by Sir Gilbert Blane, Bart.

Sackville Street, March 22, 1832.

The adjudication of the Medals, ought in conformity to the directions laid down in the preceding Plan and Regulations, to have been made and promulgated on the 12th of August last, but it has been hitherto delayed in consequence of the want of punctuality on the part of the artist employed.

The Journals, in number 538, delivered into Office in the four years preceding the twelfth day of July last, having however been examined by the Medical Commissioner, and he having referred nine of those Journals for the inspection of the Founder, he now communicates to the Medical Commissioner his decisions thereon.

The names of the nine Surgeons whose Journals compose the primary selection are as follow :

Dr. John Liddell of the Asia, of 84 guns.

Dr. William Donnelly, of the Hussar, of 46 guns.

Robert Purkiss Hillyer, Esq. of the Albion, of 74 guns.

Dr. George Grant of the *Forte*, of 44 guns.

William Martin, Esq. of the *Java*, of 52 guns.

Dr. John Wilson, of the *Rattlesnake*, of 20 guns.

Dr. John Hamett, of the *Volage*, of 28 guns.

John Houston, Esq. of the *Erebus* Bomb.

John Tarn, Esq. of the *Adventure*, Surveying Sloop, of 6 guns.

Out of these, the Founder has selected as the successful Candidates for the Prizes, the two following :

Dr. John Liddell, of the *Asia*, and

Dr. William Donnelly, of the *Hussar*.

It may possibly be alleged in disparagement of this Foundation, that never was there a period in the History of Great Britain, in which an institution of this nature seemed to be so little required, inasmuch as there never has occurred a period in which the preservation of the health of mariners has been so well understood, nor so skilfully and vigilantly attended to. But on the other hand may it not be alleged, that the more valuable these recent methods are, the greater should be the anxiety that they do not pass into desuetude, for we learn too truly from history, that there is a proneness in human nature to retrograde in virtue and patriotism, as exemplified in the degeneracy of Greece and Rome. But there appears a still more cogent reason why the excellent salutary practices so lately discovered

and now in operation should never be forgotten ; namely, that by virtue of the modern improvements in health, war can be carried on by the expenditure of one half of human life and treasure, as will appear from the following brief statement. The scurvy has nearly vanished, that peculiar pestilence of a life at sea, and of which Sir Richard Hawkins said, in the time of Queen Elizabeth, that he had known of ten thousand mariners having perished in twenty years, at a time when the marine of England was not one twentieth of what it now is. Fevers are nearly extinguished ; there is a prodigious diminution of dysentery, and a comparative absence of ulcers. Of these, scurvy caused the greatest embarrassment to the service, for formerly a fleet could not keep the sea for more than seven weeks, whereas it can now do so for an indefinite length of time, so as not to require more than one half of the number of seamen. The means by which these great ends have been compassed, have been superior medical and surgical skill, the introduction of lemon and lime juice, rigorous attention to cleanliness and ventilation, the suppression of intemperance, some improvement in diet and clothing, to which may be added a superior attention on the part of Commanding Officers to all those points of discipline which bear upon health. Ulcers are a species of evil which were formerly not so much noticed on account of their being

less frequent than some other naval complaints. They have however on various occasions proved serious embarrassments to the service, and matter of extreme suffering to individuals. Their mischief is also greatly aggravated by their being infectious, as was clearly proved to the Founder, while Physician to the great West-India Fleet of near forty sail of the line, in the American war; and when a Commissioner of Sick and Wounded Seamen in the late war, in his examination of Journals. He once knew them to be introduced and to spread in a ninety-gun ship, by a single case being brought on board. They spread in a ship employed in carrying invalids from Cork to the channel, among whom were several cases of ulcer; and other proofs could be adduced.

This diminution of expense must be highly consolatory and forcibly striking to every patriotic member of the state, when he reflects on the present lamentable falling off of the revenue, which threatens to fall short in supplying the urgent necessities of the country even in peace, and how much more in war, or the prospect of it, when the dignity and defence of the Empire may be compromised.

The Founder having stated these preliminaries proceeds to put on record the main reasons which have guided his judgment in making his award.

Dr. Liddell, who stands foremost of the successful candidates, was surgeon of the Asia of 84

guns, the flag-ship at the Battle of Navarino, on the 20th of October, 1827, and what strongly attracted the Founder's notice in perusing his Journal were the arrangements preparatory to that Battle, which were not only extremely judicious, but argued a considerate and anxious foresight under the dictates of a calm and rigorous sense of duty.

This point of the surgeon's duty has not been so much commented upon by authors as might have been expected, considering the very interesting nature of its circumstances. But Dr. Liddell's narrative, his description and recommendations, though so conspicuously excellent, have not all of them the entire merit of novelty; for Dr. Quarrier, surgeon of the *Leander* in the Battle of Algiers, on the 13th of September 1816, has published in the Transactions of the Medico-Chirurgical Society of 1818, a most instructive and well written account of the like incidents, and which have been usefully enlarged and commented upon by Mr. A. Copland Hutchison, in the Second Edition of his *Practical Observations on Surgery*.*

Dr. Liddell has nevertheless much merit in

* From dates, and upon enquiry, I find that the surgeons of the expedition were equally acquainted with the opinions of Mr. Guthrie and Mr. Hutchison on the point in question, before Sir Edward Pellew's squadron sailed from England.

following up these points of practice with great zeal and good judgment.

On both occasions there was a wide scope for the exercise of eminent skill and humanity, owing to the sanguinary and tragical nature of these conflicts, the only events of the like nature since the general pacification of 1815.

These preparatory measures consisted in the considerate and wise regulations, in the provision made for the ready administration of assistance, not only as respected instruments, dressings, and the timely stoppage of the effusion of blood, but for the supply of cordials, and the means of procuring the greatest ease in the movements and postures of the wounded. There is in Dr. Quarrier's paper a description of a method for lowering the wounded from the upper deck to the cock-pit by means of slung cots, not adverted to by Dr. Liddell.

Dr. Liddell, on the other hand, employed a new method of forming a commodious amputation table, by the new contrivance of placing the boards of the mess-tables on the midshipman's chests.

On this occasion the Founder takes leave to remark, that it has surprised him a good deal not to have met in any book of Surgery with any mention of what is the most advisable method of saving life in cases where it is immediately endangered by a profuse hæmorrhage, whether by sea or land.

What is here meant will be best understood by the affecting narrative of a case which occurred almost immediately under the Founder's eye, having happened in an action on the 9th of April, 1782, precursory to the memorable conflict on the 12th of that month, and which made a deep and lasting impression on his feelings. He was then serving as physician to the fleet on board the flag ship.

Captain William Bayne, commanding the *Alfred* of 74 guns, had on that day a leg carried off, and the hæmorrhage was so sudden and profuse that he expired before those who bore him could reach the cock-pit.*

It has often since occurred to the Founder, on whose mind this incident has long and seriously dwelt, that had the effusion of blood been prevented, by applying on the quarter-deck a tourniquet of a construction ever so rude, such as the writer of this carried in his pocket; and had a strong cordial been administered to him at the foot of the cock-pit ladder, his valuable life might possibly have been saved. It may here be remarked, that simple tourniquets are wisely directed by the naval regulations to be supplied to

* Captain Bayne was under the middle size, of a full habit, and ruddy complexion, which probably added to the suddenness and copiousness of the hæmorrhage. He was not only an excellent officer, but had a great turn for scientific mechanics, which was found occasionally useful in carrying on the duties of the ship.

the tops in the time of action, on account of the distance from the cock-pit, and also in boats when sent on hazardous service. The best cordial he believes to be a compound of brandy or other alcoholic liquor and laudanum, in a dose of about two fluid ounces of the former to half a fluid drachm, or even more, of the latter; and the writer humbly suggests whether it would not be an advisable regulation that in time of action there should be ready prepared a bottle of this compound liquid in the cock-pit of a man of-war, or in the rear of an army.

Another remark arising out of the present subject is, that the incidents which occurred in the two actions afforded some practical proofs for the decision of an important question in naval and military surgery, namely, whether in cases of those so grievously wounded as to require amputation, it is most expedient to perform that operation immediately, or to defer it. It is a point which has been strenuously argued between Mr. Guthrie, of the army, and Mr. Copland Hutchison, of the naval service. The former, with those who followed his practice, contending that it was most advisable to defer it for several hours, even eighteen or twenty-four; the latter maintaining that the great advantage lay on the side of immediate operation.

The question having been amply and liberally argued on both sides, it was made clear to the

universal conviction of the profession that the preference was due to the immediate operation.

And it must not be omitted in this record to mention that Mr. Guthrie, in that true spirit of candour which becomes a gentleman deservedly at the head of the army practice, has given his unqualified assent to his antagonist's opinion (see the second edition of Mr. Guthrie's Publication.) His writings ought to be in the hands of every military and naval surgeon, as well as the writings of Dr. Quarrier and Mr. Hutchison,—from the whole of which the decided conclusion is as here stated. The most powerful argument is, what must in all practical subjects be the most unanswerable, the matter of fact. In the *Leander* and *Severn*, in the battle of Algiers, the immediate amputations were attended with the most eminent success, while the result was quite opposite in those in whom the deferred method was adopted. The very eminent Baron Larry, Surgeon General to Bonaparte's armies, is the advocate of the same opinion.

To any one, even though not belonging to the profession, it cannot but be obvious that by this decision of the question there must be a saving of the most intense and protracted agony, not to mention mental anguish; for what can be more clear to common sense than that in the nature of things, the most acute sufferings must be unavoidable from the grating of the rough points

and edges of the splinters of wood and fragments of bone on lacerated nerves,* which nothing but the removal of the limb can obviate. And it is a matter of grave consideration that unbearable sufferings also arise from the protracted application of the tourniquet, insomuch that the poor creatures have been known to tear them off and bleed to death.

Before quitting the subject of the cock-pit it would be injustice to Sir William Burnett, the present sole Medical Commissioner of the Navy, not to mention that he has so methodized the arrangements for ordinary service, as well as on pressing occasions, as to render them far superior to the former ways of administering these duties. There seems to be nothing wanting but some well devised contrivance for securing ventilation and

* There was a degree of pertinacity amongst the advocates of the opposite doctrine which probably took its rise in the widely diffused opinion which the Founder heard delivered fifty-four years ago, in the course of the Surgical and Anatomical Lectures of Dr. William Hunter. This very eminent teacher observed that in cases occurring among men whose constitutions had been brought low, and their bodies emaciated and weakened by long chronic complaints, such as Chelsea and Greenwich pensioners, bore amputation better than those in whom the same operation had been rendered necessary by severe mechanical injury, while in the height of health, strength, and youth. It is easily conceivable how young men, and the Founder himself no doubt among the rest, might mistake and confound these cases, so as to influence their future opinions and practice.

coolness, for it appears in the narratives of the Journals, particularly that of the *Leander*, that during hot weather the sufferings of the wounded were extremely aggravated by the close heat. A wind-sail being best known is the resource which most readily suggests itself, but the supply of cool and fresh air is much better provided by a tube proceeding from below, terminating a little above the upper deck, and surmounted by a sort of vane, called a cow. Both contrivances ought to be adopted. About thirty years ago the Founder was consulted by the Secretary of State to recommend some means for preserving the life and health of convicts on their passage to Botany Bay, very few of them having survived on former voyages, and such was the success of the means above recommended, that these convicts male and female, remained quite healthy, and a great part of the benefit was believed to have arisen from this plan of ventilation (See volume i. page 38).

There is but one other point having relation to wounds, and which may be adverted to, more as an object of curiosity than much utility, namely, the divided opinion regarding the *wind of a ball*. The discussion respecting it, seems to have arisen chiefly from a perverted statement of the question; for the question is not so much whether there be any reality in the appearance of the injured parts being caused by the mechanical

impulse of the imaginary wind of the ball; as it is, whether this obscure and difficult problem may not be solved by some other and better ascertained cause than this compression of the atmospheric air. The Founder has considered the subject perhaps more than it deserves, as may be seen at the 575th page of the third Edition of his *Observations on the Diseases of Seamen*, in which there are some circumstances not mentioned in the Journal but too long for insertion here.

The passages of this Journal which have been thus far commented upon, relate only to surgical practice. Before entering on the medical part of the Journal, the Founder will submit some remarks on the professional education of Naval Surgeons; though he himself has no practical knowledge of this branch of the art of healing.

As the surgeon of a ship of war is the only person to whom recourse can be had, whether in sickness or in wounds, in the midst of the ocean, or remote regions of the globe, a situation as interesting as a human being can well be placed in, a more complete system of education seems to be required of him than in any other situation in which a professional man can be employed. His duty indeed lies more in sickness than in wounds, except perhaps after a battle. Yet such has been the neglect of proper education, that during twelve years, in which the Founder was physician

to one of the largest hospitals in London, it grieved him to the heart to see numbers of young men from the country frequenting it, in order to learn what they could, but spending their time and means in what is vulgarly, but properly enough called, walking the hospital, that is (with the exception of a few who paid exorbitant fees as dressers,) merely looking on to see operations and dressings, and never bestowing a thought on acquiring medical knowledge. And it is painful to the Founder to mention that those who superintended the education deemed it no part of their duty to guide their studies aright, allowing them to believe that walking the hospital, and attending a course of anatomy, sufficiently qualified them to enter upon practice, whether in physic or surgery, by sea or by land. The Founder was baffled in his attempts to remedy this. What little practical knowledge those acquired who immediately after, either settled in civil life or went into the army and navy, was during their apprenticeships with experienced general practitioners. It is therefore wisely prescribed by late regulations that the candidates for the Navy be required to bring certificates of having attended Medical Lectures and Medical practice of hospitals. This accounts for the Navy being now supplied with better Medical officers than formerly, though partly owing also to there being a much greater number of Candidates for selection in Peace than

in War. The Founder can indeed bear testimony to this, from the able medical practice exhibited in the nine selected Journals which have been submitted to him on this occasion, and in proof of this he proceeds to offer some specimens extracted from those Journals.

The first point of practice which attracted his notice in the journal of the Asia, was the judicious employment of opiates according to some new modern methods of practice, in which their utility has been demonstrated in several complaints. And it is singular enough that some of their uses were better known to the ancients than to us; so that the modern application of them may be more properly deemed *revivals* than *discoveries*. What is here alluded to is certain compositions handed down from the classical ages which owed a great part if not the whole of their virtue to opium. They are still known under the titles of Theriaca, Alexapharmica, Mithridatium, Confectio-Andromachi, Philonium, &c. the word opium itself is indeed proof enough of the estimation in which it was held being merely what grammarians call an augmentative, the Greek word *Οπος*, Succus, Lat. Juice, Engl. as we say emphatically Bark, meaning Peruvian Bark, *κατ' ἐξοχην*. Its virtue, as a remedy in Agues as an exhilarant, as a sovereign remedy in Diabetes, and in the cure of malignant ulcers, are matters of recent improvement in practice. The Founder had found

it a powerful subsidiary to alkaline remedies in effecting a cure of the red or lithic acid gravel, and for rendering it more permanent when effected; and it hardly admits of a doubt that the *Nepenthe* of Homer was opium.* These compositions were improved no doubt by their combination with tonics, aromatics and other ingredients. But this incongruity is no reason why moderns should have ignorantly stigmatized these multifarious combinations, by the opprobrious appellation of *absurd farragoes*. What has led the Founder here to notice this subject, is a passage in the Journal of the Asia, from which it appears that Dr. Liddell, was not unacquainted with the virtue of opium in the cure of agues. The merit of the discovery is believed to be due to Dr. James Lind of Haslar Hospital; but the mode of its application has since been greatly improved, for Dr. Lind recommended it to be given during the hot stage of the paroxysm; whereas it has been since found much more advisable to administer it half an hour at least before the accession of the cold fit, in the dose of about half a fluid drachm of the tincture of opium, along with half a drachm of the powder of rhubarb, or about half a fluid ounce of its tincture, to prevent the constipation consequent upon opium, as well as to improve its

* It is within the memory of the Founder, that opium was seldom if ever employed but as an occasional soporific and anodyne.

virtue. The writer of this during the latter part of his practice, has made but little use of the Cinchona in the cure of Intermittents, trusting almost entirely to this remedy with suitable tonics in the convalescent state, such as the Cinchona and some of the preparations of iron, and he flatters himself that a more general adoption of its use in this disease, as well as an exhilarant, and for the cure of malignant ulcers, it will become more general when better known. He has occasionally found great benefit by combining with each dose of the opium, from six to ten minims of the *Liquor Arsenicalis* of the Lond. Pharm.

The Founder will only add a few words respecting its introduction into practice in the cure of these ulcers. In the year 1776, when he first came to London, he learned from Dr. Nooth, appointed that year Physician General to the armies in North America, that he had met with a remarkable instance of the cure of a phagedenic bubo which was considered as hopeless, by the free use of opium. He found this confirmed in the exercise of his duty in America. And when the Founder was Physician to a great Fleet in the West Indies, and North America, in 1782 and 3, most deplorable complaints were made of the dreadful sufferings of the individuals, and of the embarrassment to the service from malignant ulcers. Neither Cinchona nor any other remedy seemed of the least avail, but recollecting what

he had learned from the very eminent and learned Dr. Nooth, in England and America, he recommended with most satisfactory results the treatment by opium, along with all those precautions which might prevent them from communicating by contagion, as they evidently did. The cause of truth and humanity compels the writer of this to animadvert upon the neglect which these points of practice have met with from the profession at large. The writer concludes with saying that in his opinion there is not a *desideratum* in practical medicine, more important and interesting than a Monograph Treatise on Opium; and that there are some late chemical preparations of it greatly superior in many constitutions to this drug in its natural state.

The next subject of commendation in medical practice occurring in this Journal is the treatment of bowel complaints. There was a considerable number of severe cases of dysentery, enteritis, and of that species of cholera which is commonly endemic and periodical in England, and not the Indian, now so greatly the subject of alarm.

The Founder has elsewhere observed, that of the inflammations which occur in the three vital cavities, it is in those of the abdomen in which there is the greatest room for gratulation on successful practice, as contrasted with those of the brain and lungs. Early, energetic and judicious

means hardly ever fail to succeed. By virtue of free bleeding, both general and local, free purging, emollient clysters, and a very large blister to the abdomen, the Founder has lost only one case in forty years practice in abdominal cases. Dr. Liddell's practice was eminently successful in the like cases, and by the like treatment.

But there does not appear either in this or any other of the select Journals, a single example of the severe, dangerous and most excruciating of all abdominal complaints incident to temperate climates, namely, the *Ileus*, otherwise called *Iliac passion*, and sometimes *miserere*, from the plaintive tone emitted under the agony of the sufferer. A case occurred in the practice of the Founder which may not be thought out of place here shortly to relate, as well from its interesting and instructive nature, as from the illustrious rank of the patient, as a specimen also of the modes of prescription at this era. He was fifty years of age. The writer's attendance was required on the 6th of August, 1816, at a royal villa, at the distance of twelve miles, at five in the evening. On his arrival he found that the patient in the morning of that day soon after breakfast, was without any assignable cause seized with severe pains in the stomach and bowels, and which continued increasing in the course of the day. There was violent and incessant vomiting of a semi-fluid stuff of a brownish yellow colour, which was

the feculent matter; no doubt characteristic of the complaint, and though a few grains of calomel and cathartic extract, also a clyster had been given, there was neither relief, nor any evacuation *per anum* procured. These, and a small bleeding from the arm were the only remedies had recourse to. The pulse was soft, intermitting and fluctuating from 50 to 112. The following remedies were immediately prescribed.

℞. Hydrargyri submuriatis ʒij. pulveris Tragacanthi comp. ʒj, misce bene terendo et divide in chartulas quatuor equales. Sumatur una statim et repetatur quartâ quâque horâ. ℞. Aqua. Menth. Pip. fluid. unciam unam et dimidiam Magnesiae sulphatis ʒij. Magnesiae subcarbonatis Sacchari albi āā ʒij. Misce; sumatur duas horas post singulos pulveres in ebullitione cum cochleari uno magno succi Limonis.

Fourteen leeches were ordered to be applied to the abdomen, being all that could be procured; profuse bleeding indeed is not so necessary as in some other severe intestinal complaints, for this is essentially more spasmodic than inflammatory. A large blister and a warm bath were ordered to be in readiness. The stomach retained the medicines, the vomiting having ceased after the first powder, and did not return. The bowels were relieved by copious evacuations soon after the third powder had been given; and the medicines already prescribed having been administered, pro-

cured such complete relief, that early next day the Founder felt himself justified in announcing to the illustrious family to which the patient belonged, that he was in a state of safety.

The only other practical remark to be made on this Journal relates to *Digitalis* as a febrifuge. This idea was suggested, no doubt, from its retarding the pulse, on the supposition that frequency of pulse is the main constituent or proximate cause of fever, whereas according to the soundest opinions it is only the effect or a symptom of it. But this is not the chief ground of the Founder's doubts, for he has not practically found it a febrifuge remedy. It is, however, a very valuable medicine in Dropsy, particularly Hydro-Thorax.

Dr. William Donnelly, of the Hussar, of 44 guns, was the other successful candidate.

The most prominent branches of practice which attracted the notice of the Founder in the perusal of the Journal of this ship were what he met with regarding the nature and treatment of syphilis and acute rheumatism.

The characteristic symptoms of the former are commonly divided into those belonging to the primary and secondary stages; but there are two other aspects of it which have not (as far as the Founder's erudition and knowledge extends,) been sufficiently adverted to by authors and practitioners. By one of the aspects is meant the

initial forms of it, consisting in gonorrhea, excoriation, and the relation in which these stand to the secondary forms of the disease. By the other aspect he means certain symptoms which may be termed *sequelæ*, such as phagedenic bubos, and various other corrosive and destructive ulcers, alleged by some, but denied by others, to be of a syphilitic nature, and rather injured than relieved by the employment of mercury; and there is the like ambiguity regarding the employment of it in the other aspect.

These unsettled points (*questiones vexatæ*) are of great practical importance, and have been discussed by Dr. Donnelly in the spirit of elaborate research and discrimination; and he has exhibited various comparative statements upon a large scale, by throwing them into the form of tables, and has reasoned upon them in a judicious and argumentative manner. The practice also is that which was best known before the virtue of opium in certain untractable ulcers was discovered.

The other remark above alluded to relates to acute rheumatism, numerous cases of which arose from the vicissitudes of weather and climate, and the deficiency of clothing. It so often baffles us that it may be almost deserving of being enlisted among the *opprobria medicinæ*. But are we not compelled to confess that our want of success depends as much on erroneous practice as on the

nature of the disease; and that this error consists in excess of bleeding, indicated by its apparent analogy with febrile and inflammatory complaints. But it will appear on close consideration that these analogies are more than counteracted by certain discrepancies, the most important of which is that the coriaceous surface of the crassamentum, which in all cases really inflammatory is cupped, but in acute rheumatism is more tenaceous and thicker, but horizontally plain and co extended with the upper surface of the blood. It is clearly therefore a morbid condition of the blood, differing essentially from that affection of it which constitutes proper inflammation, and whereby new muscular fibres, vascularity, and the adhesions of membranes, are created. Nor is it characterized by the typical and periodical remissions and exacerbations belonging to pyrexial affections. Neither is it attended with the same sensorial symptoms as fevers, whether continued or intermittent. But whether these views are just or not certain it is, as a matter of practical fact, that in the very great majority of cases excessive and indiscriminate bleeding is highly pernicious.

A strong proof of the advantage of an opposite practice was adduced by Dr. Dawson, a very eminent practitioner of the last century, who published to the world the very great success he found from the use of the volatile tincture of guaiacum (*tinctura guaiaci ammoniata*, Lond.

Col.) in doses of half a fluid ounce. This seemed so abhorrent to reason, in a disease where the prominent symptoms were heat and irritation, that it did not meet with a favourable reception. And the writer of this, in common with others, after some superficial trials laid it aside. But it has been found more successful by us in later times in certain cases, *particularly* in scrofulous constitutions, and the fluctuating diathesis of individuals. Another striking peculiarity in the treatment of this disease, apparently incongruous also with its nature, is the success which has been found to attend the treatment of it by Peruvian bark. This has been established by that very eminent teacher, Dr. George Fordyce, and other practitioners of high character. And it may be inferred generally from these remarks, that new and unpromising modes of treatment ought not to be too hastily rejected. The Founder has been led to make these remarks from observing that Dr. Donnelly, surgeon of the Hussar, employed the bark successfully in acute rheumatism. There were in his journal several cases of articular gout which terminated favourably, but the duration of which would, in the writer's opinion, have been considerably abridged by the use of colchicum. The only other practical remark that need be made is the successful treatment in cases of worms, which leads the Founder to mention an ingenious and useful doctrine, which he heard

delivered *vivá voce* by Dr. William Hunter, and which he believes has never been published. It was that these animals, being usually found in children of weak *stamina*, and engendered by vicious digestion, were rather to be considered the *effect* than the *cause* of the indisposition, and that though evacuation was quite necessary to carry off the *colluvies*, a radical cure could only be effected by strengthening the constitution, and the powers of assimilation, through appropriate diet and tonic remedies.

It would be too tedious to insert here a minute recital of Dr. Donnelly's sound and correct practice ; but it is a matter of justice to Mr. Kennedy, his assistant, to state, that the report of an examination of an organic affection of the *heart* and *aorta* was drawn up by him with much anatomical precision, indicative of superior education.

The Founder having thus pointed out the principal points of merit which directed his judgment in favour of the competitors, feels it as a matter of painful and serious regret that there should not be a greater number of medals, or other tokens of approbation, for other candidates who have clearly and fairly earned distinction by their great skill, diligence, and humanity. He cannot too much admire the indefatigable and judicious exertions of Mr. Purkis Hylliar, of the Albion, whether in the treatment of the wounded at Navarino, or of the medical cases. The following

interesting and ingenious remark occurs in his Journal: "On the morning of that day (the 20th of October, 1827,) there were twenty-eight on the sick list, very few of whom again presented themselves, and the ship has been uncommonly free from sickness ever since." This unusual degree of health was no doubt ascribable chiefly to the excitement of battle, but partly also to himself and the officers, by their exemplary attention to cleanliness and ventilation, from which causes, and the fine climate, health might have been still better preserved had it not been for the pulmonic complaints caused by that deficiency of clothing so often complained of by other surgeons, a grievance which the Founder humbly submits to the consideration of those in authority. He hopes the like difficulty in effecting this as happened to the Founder, in procuring a supply of soap, will not occur. Little are those in supreme power aware how much health, life, and treasure are saved, how much efficiency of service is promoted by means apparently so insignificant, and attended with so little expence.

Dr. Wilson, of the Rattlesnake, has shewn great judgment and accurate discrimination in the general treatment of his cases. The most interesting of these were two cases of neuralgia, or rather three, for there was a relapse of one.

They both soon yielded to ten grains of calomel, and as much antimonial powder combined with

two grains of opium ; the case of relapse obtained a permanent cure, which might be fairly enough imputed to the addition of two fluid drachms of oil of turpentine ; and Dr. Wilson remarks that the superior efficacy of this addition was better evinced by its occurring in a case of relapse than if it had happened in two separate subjects, which might have been ascribed to a difference of predisposition.

Mr. Martin, of the *Java*, gives an interesting account of the Indian Cholera, of which there occurred thirty-nine cases, of whom eleven proved fatal. He observes, with becoming modesty, how difficult it is to form a satisfactory opinion regarding its remote cause, on account of its singular anomalies compared with other epidemics. These consisted in a much smaller proportion of those who are exposed to it being actually seized with it than what occurs in other popular diseases, meaning, no doubt, the small pox and the plague ; also in its sparing no age nor sex, nor being controuled by weather, climate, or season. They brought it from the Coromandel Coast to Bombay, where they lay for some time at anchor, and then sailed for Penang.

During this period it was remarkable that many ships in company with them, both in port and at sea, remained free from it. This seems to prove clearly that it was not in the air, but must have been communicable, especially when it is consi-

dered that at land it has been found everywhere to spread *progressively* and not *simultaneously*, so that no living creature could avoid inhaling its venom had there existed any in the general atmosphere. It is quite intelligible how a few individuals can be picked out from a multitude by breathing the breath of the diseased, but quite inconceivable how this could be the case were it in the atmosphere, which is every moment breathed by every human being. It can also be every where traced to human intercourse, except at Jessore, where it was no doubt generated by such a concurrence of circumstances as has been known to engender new infections in so many other instances.

Mr. Martin's treatment, besides following the most approved method known, was sagaciously, ingeniously, and successfully varied, by the internal use of the oil of turpentine in two fluid drachms, which brought away feculent conjestions and worms. It may be incidentally remarked, that though the internal use of this remedy is not new, it has of late been employed in such large doses, and in such a variety of diseases, that it may be reckoned so; for during this time it has been exhibited in doses from one to two ounces with eminent success in cases of tape-worm and lumbricus. In the former cases the Founder has prescribed it successfully from ten to twelve drachms, qualifying it to the stomach

with a due addition of spirit of lavender and syrup of orange peel.

Dr. Hamett, of the *Volage*, has not been behind his brother officers for his good judgment, diligence, and amiable solicitude in the treatment of those under his care. These qualities were particularly exemplified in his persevering attentions to a private gentleman at Lima; and might not this be adduced as a proof how little injurious, nay, how beneficial, private practice* may be, if occasionally permitted, to those engaged in the public exercise of the profession?

Dr. George Grant of the *Forte*, has made some ingenious observations on the venereal disease, with a view to discriminate those which require to be treated by mercury. He relates a curious circumstance, which coincides with a remark made by the Founder. Some young ignorant clowns, conscious of having exposed themselves to the venereal infection, could not be convinced of their not labouring under it even by their medical attendants, who found no symptom of it; but so obdurately fixed was this persuasion in their minds, as to excite imaginary pains in their loins, as they affirmed, for which there was no cure but that of subjecting them to a course of mercury, sufficient to produce a slight salivation. They were even allowed to dictate their own

* For the Author's opinion on this subject see a Letter written in 1824 in the Appendix.

method of cure, which was by unction, or *laying down*, as they called it, affording a curious moral phenomenon of the human mind, and extensive practitioners occasionally meet with similar fancies, even in persons of education. Dr. Grant shewed a like indulgence to these cases. But the most interesting case related by him is that of a severe and obstinate suppression of urine, which he treated with the most anxious attention and practical expertness in puncturing the bladder, by which life was evidently saved.

If there are no cases important enough for animadversion in the sloop and bomb, let it be imputed to the more limited scope of practice afforded by so small a number of subjects, and not to any deficiency of talent or education. Mr. Houstoun, of the *Erebus*, has treated his patients very skilfully, particularly a case of hæmoptoe. And the like may be said of Mr. Tarn, of the *Adventure*, in which vessel a number of pulmonic complaints occurred, owing to the vicissitudes of season and climate, without sufficient clothing to counteract them. The Founder may now therefore with great truth and exultation congratulate the British Navy and Nation on possessing so many accomplished and well educated Medical Officers, for the more the subject is considered, the more will it appear undeniable how momentous the state of health is to the efficiency of that great national bulwark to which Britain

owes her prosperity and her glory. And needs there any better proof of this than the historical evidence adduced by the Founder in his late statement of the health of the navy for the last thirty years ; from which it clearly appears that war could now be carried on at one half the amount of men, tonnage, and treasure, by which it was carried on fifty years ago ; and it is further demonstrable, that without the improvement of health all other improvements, whether consisting in sheathing with copper, the oblique frames, and other parts of naval architecture, conducive to ventilation and dryness, introduced by Sir Robert Seppings ; also the great improvements in navigation by the profound invention of astronomical science ; all these it may be safely affirmed would have been quite nugatory had it not been for those amendments in health which now enable mariners to keep the sea for an indefinite length of time.

The Founder concludes with remarking that there reigns throughout these Journals a fine vein of commiseration for human suffering with an ardent desire of relieving it. It is not easy indeed to conceive a more favourable field for the most eminent and amiable duties of human life, nor one from which may be expected a richer reward in old age and retirement in reflecting on the pure and inexhaustible source of delight springing out of the recollections of a well spent

life. The following is a vulgar apophthegm meant to comprise the requisite accomplishments of a skilful surgeon. "That he should possess the hand of a lady, the heart of a lion, and the eye of a hawk;" but requisite as these are, what are they in point of dignity and efficiency compared to those sublime and indispensable qualifications, the practice of which the greatest of philosophers and orators* pronounced to be the points in which man most resembled the Divinity; and may not this be most of all applicable to Naval Medical Officers, acting as they do under their solitary responsibility, with an exclusive reliance on their own resources. Nay, was it not the exercise of the healing art which formed the favourite field of those miracles, selected by its author for the establishment of the Christian faith? The writer of this as a humble member of that profession, to the study and practice of which he has addicted himself for sixty-three years, and unaffectedly conscious on a retrospect of his very long life, that he has not on all occasions, done all the good he might, and ought to have done to his fellow-creatures, wishes he could without arrogance and with a better grace thus take upon him to admonish others.

Having arrived at the eighty-third year of his age, and labouring under a variety of serious infirmities, with little hope of again performing

* Cicero.

the like duty, he will now with the warmest sentiment of unfeigned regard, and best wishes for the continuation of the respectability and welfare of the Medical Officers of the British Navy, only say to them,

Valete

Vixi.

APPENDIX TO DISSERTATION XIII.

Sackville-street, Jan. 25, 1824.

MY LORD,

I now comply with the wish which your Lordship was pleased to express to me in conversation a few days ago, that I would state in writing the question relating to the expediency of interdicting private practice to the Medical Officers of Naval Hospitals, Dockyards, and Marine Infirmaries.

I begin with remarking that in so far as I can learn, this subject had not been matter of express instruction till about the time of the peace of Amiens, except that in the instructions given to Governors of Hospitals at their first appointment in 1795, they are directed to permit private practice in time of peace;* and in the general printed instructions of 1746, the Physicians to hospital ships are required “not to demand any fee or reward from those under their care;” but this evidently relates to persons belonging to the service. A regulation originating in the Board of Admiralty was made in 1802, that all Medical Officers of Naval Hospitals, whether in peace or war, should abstain from practising their profession among the civil community. This came afterwards under the consideration of the Commissioners of Naval Revision, and being approved by them, was incorporated into the code of general instructions now in force, with a penalty annexed to it, of a severity equal to that which is inflicted on criminal acts, namely, a mulct to the amount of three times their yearly salary, besides an oath and a bond, and “in case of default to be dismissed the service, and rendered incapable of serving his Majesty in any civil capacity.” The only qualifying clause to this is

* This is quoted from the evidence taken by the Commissioners of Revision in November, 1803.

“except in cases of sudden and accidental emergency ;” but the acceptance of any remuneration is strictly prohibited *in all cases*.*

Those who may be inclined, at first sight, to find fault with this severity, ought to recollect how indispensable it is at Naval Hospitals in time of war, that medical officers should be constantly at their posts ; for it not only happens occasionally that these hospitals are so full as to afford occupation for their whole time, but the nature of this service is such that ships may unexpectedly arrive filled with sick or wounded men either from battle or from long voyages or cruises, and as there may be medical officers so unprincipled as to suffer their private and selfish avocations to interfere with the sacred and paramount duties of their public functions, it has been judged advisable to ensure and enforce the strict performance of them through the efficacious agency of the above cited penalties.

As the detriment to the service from the private practice of physicians and surgeons did not formerly attract the notice of the public authorities presiding over the Navy, it is presumable that the inconveniencies arising from it, had not been glaring either in frequency or magnitude ; though from the infirmities incident to human nature, neglect must occasionally have occurred, whether from private practice or other causes. In accounting for the late rigorous regulations, it will hardly be alleged that the character of this age has degenerated in point of virtue and honour, particularly among the liberally educated classes of society, to which medical officers belong. During the seven years in which I belonged to the Medical Board of the Navy, I cannot recollect more than two instances in which it was alleged that the service had suffered from this cause. One of them was that of one of the principal Medical Officers of a great Naval

* See the Instructions of 1808, pages 71, 73, and Appendix No. 2.

Hospital, who could not be found at a time when a considerable number of sick and wounded men were unexpectedly brought there. It was found, however, upon investigation, that he had not been engaged in professional visits, but had gone a few miles into the country for recreation. The other was of a very different description, being that of one of the Surgeons* who most reprehensibly, when on the point of performing a capital operation, deferred it for two hours in order to comply with a call made upon him at that moment by a private person. A third case was stated to me by the First Lord of the Admiralty, with whom I had the honour of a conference on this subject in the year 1802. The object of his Lordship's animadversion was one of the Naval Physicians at Plymouth, who, he said, was notorious for his neglect of duty from his attachment to private practice. But so ill informed was his Lordship on this matter, that the physician alluded to, being a man of independent fortune, never sought nor would even accept of private practice. This physician was a gentleman of considerable learning, and remarkable for his great attention to the cleanliness and ventilation of the hospital, as I witnessed when there in 1781, but he was frequently absent on his private affairs, and lax in his personal attendance from indolence, and perhaps from some want of that strong sense of duty and of that taste for his profession, which ought to make part of the character of every medical practitioner.

But besides indolence, there are several other causes which

* This surgeon was not taken from the list of Navy Surgeons, but from the civil practitioners of Plymouth. This is the only case adduced by the Commissioners of Revision, as the ground for recommending indiscriminate restriction from private practice. See Report, 4th Nov. 1803, and a Member of the Commission has since informed me that it was this case which decided their judgment.

disqualify Medical Officers for their duty, such as incapacity, intemperance and other dissolute pleasures, to which may be added private practice. But from whichever of these causes neglect of duty may proceed, it does not seem necessary that they be specified; for I humbly apprehend that the good of the service merely requires, on the part of those who superintend it, that every officer be seen at his post in the undisturbed and efficient performance of his duty, and that in case of neglect he be either reprimanded or dismissed according to the flagrancy and frequency of his offences. In this view of it, the Governor or other superintending officers, would also be spared the humiliating task of exploring the private haunts, and prying inquisitorially into the manner in which the Medical Officers spend their vacant hours. And it is remarkable with regard to the regulation alluded to, that, of all the causes of neglect above enumerated, private practice is the only one specified, though it is certainly not one of the most reprehensible, immoral, nor the most frequent. On the contrary, though there may be instances of unprincipled men who have neglected their duty for their own sordid ends, there are certain advantages which do accrue to the service from such exercise of professional talents as the public exigencies will admit of.

First, through the additional experience and skill thus acquired, by which the practitioner becomes a better Physician or Surgeon to the Hospital or Dockyard. In the great civil Hospitals of this metropolis, (to one of which I was physician for twelve years) if any candidate for a vacant situation were to try to recommend himself by declaring that he would devote his whole time and attention to the duties of the Hospital, he would, to a certainty, be rejected; for the Governors, as men of good sense, would be well aware that he could not find employment to occupy him the whole day within its walls; and nothing can be more obvious than that the hospital would profit by the private experience of

the professional man, as much as the private individual would profit by the experience acquired at the hospital.

Secondly. It cannot be denied that at some rare crisis, such as the action of the 1st of June, 1794, there would be sufficient occupation for medical officers for nearly the whole day ; but it is to be hoped there are few or none to be found in the list of naval physicians, surgeons, or their assistants, who would be so destitute of every principle of conscience and probity, of every sentiment of honour and humanity, and I will add of patriotism, as in such circumstances to desert their duty, or to absent themselves from it. The case is quite different in ordinary times ; and in time of peace the daily duties of a well regulated hospital may be performed in an hour or two at most. What is the medical officer to do with the rest of the day in case he happens not to have a taste for literature, gardening, or other becoming and innocent pastime or recreation ? Will he not be exposed through idleness to those seductions which may drive him to the tavern, the gaming-table, or other frivolous or vicious pursuits, when he might be employed in relieving the sufferings of his fellow creatures, and improving himself in his profession ? for it is by practical experience alone constantly exercised that excellence can be acquired, or when acquired be retained. It seems obvious to the plainest understanding, that on the small scale of practice afforded at the hospital in time of peace, the physician or surgeon must be retrograding, the one in his intellectual skill, the other both in his intellectual skill and in manual expertness. It is true that there are some gentlemen of such disinterested benevolence as to practice their profession* largely out of the hospital with-

*. Among these may be named Mr. Hutchinson, late surgeon of Deal hospital, and Mr. Hammick, the present surgeon of Plymouth hospital. These and other gentlemen have been offered large sums of money and valuable presents, but were prevented from accepting them by their instructions, their oath, and their bond.

out fee or reward. But will any man of sound sense and knowledge of the world allege, that this pure philanthropy is to be counted upon at the average rate of human virtue, or that it would be detrimental to the service were they to accept of remuneration, which evidently would not add to the alleged trespass on time?

Thirdly. Is there not a sort of hardship to the community, as well as to the individual, in locking up useful talent, which might be available to society, not only innocently but advantageously to all parties? This is no gratuitous assertion, for it has been seriously felt and regretted in some of the out-ports and foreign stations, that in case of severe afflictions befalling the private inhabitants, and the natural wish of having the best advice, they should be debarred from availing themselves of the gratuitous aid of the naval medical officers on the spot. In the course of the last war a gentleman of high rank and reputation, in the neighbourhood of a naval hospital being dangerously ill, he and his family desired anxiously the aid of a physician. By his instructions he could not comply, but the port-admiral wrote to the Admiralty for their leave. It was granted, but the gentleman growing worse and worse died the morning after it arrived. I was well assured of a like incident at one of the naval stations in North America, in which the surgeon of a hospital, when requested for his advice in a case of danger, in the absence of every one who could be confided in, refused his attendance, alleging that he would thereby risk the loss of his situation. In these two cases the medical officers did not seem to be aware that they might have complied with impunity provided they accepted no remuneration.

Fourthly. Another advantage which would accrue to the service from the toleration of private practice would be, that additional inducement would be afforded for able men to enter the service. And with regard to those already belonging to the service, I have known the offer of one of the situation of principal medical officer at one of the great naval hos-

pitals refused by a highly accomplished surgeon of the navy, who preferred continuing on half pay, with the prospect of a good private practice ; but he would have accepted it had there been no interdiction to the free exercise of his profession. The hospital therefore becomes a situation not worth accepting but by candidates of inferior merit ; and I have the most credible information of the serious ill effects of this in more instances than one in the practice of the naval hospitals. And might it not be added with some share of reason that a relaxation in this point on the part of the Government might be conducive to a spirit of good will and loyalty in the minds of the neighbouring civil population, in whom such harsh interdictions may beget a sullen ill humour, perhaps also thinking, and even saying, that they all contributed more or less by taxation to such institutions, and ought, though not in right, at least in the spirit of courtesy and humanity, to be permitted to participate in such share of medical talent as would not interfere with the indispensable duties of that class of officers.

Lastly. May it not be doubted whether strong expressions of distrust and suspicion, threats, denunciations, bonds, oaths, and ruinous penalties, are the most appropriate means of attracting to, and retaining in the service, the best educated men, or of securing a zealous and efficient discharge of their duty ? Does not this rather add to the other motives which discourage men of liberal attainments and character from engaging in our service, namely, the roughness of a life at sea, and the very forbidding circumstances in which an assistant-surgeon finds himself at his first embarkation ? The lowest age at which an assistant-surgeon can be admitted is twenty-two, and after an expensive and liberal education, is doomed for months, and even years, to live in the cock-pit among boys, without any accommodation for study, or opportunity of mixing in such society as he had been accustomed to. In the army the same class of medical

officers mess and associate with commissioned officers ;* and to this may be imputed the decided preference given to the land service by a great majority of the best accomplished candidates for medical employment. A further motive of preference to this service is found in the very point in question, for in the army all medical officers are, *by express instructions*, permitted private practice, with the exception of the members of the medical board.† And it is not out of place here to remark, that the efficiency of a medical officer on board of a ship is of much more importance than in the land service, for in a ship at sea no recourse can be had to further advice or assistance in case of difficulties and danger from increase of sickness or wounds.

In further discussion of this subject, it appears to me that one of the main considerations which suggested this rule to its first proposer, was the want of duly discriminating between the nature of *mechanical* and *professional* labor. It cannot be questioned that the public have a right to every moment of the time of its covenanted servants, as far as is necessary for the performance of those sacred and paramount duties with which no other engagement ought to interfere.

But it is equally manifest that the estimate of labor by time alone will apply only to what is mechanical ; the value

* I have heard lieutenants of the navy say that they would consider it a great advantage in a life at sea to have the society of liberally educated men, such as medical officers must be by the existing regulations.

† A country apothecary in Hampshire, and another at Chatham, having accused the army surgeons who were stationed in their neighbourhood of private practice, in letters to the Duke of York, His Royal Highness in answer said that he was happy to hear that any medical officers of the army were held in such high estimation, and approved much of their rendering themselves useful to His Majesty's subjects when it did not interfere with their duty to the garrisons.

of the labor of a carpenter or a clerk in office for instance, is in proportion to the time employed ; whereas the whole labor of a barrister or physician may be performed in a few minutes, for in this time he can deliver the result of the studies and attainments of his whole life. This point might be aptly enough introduced with regard to the profession of the law by putting the question whether the duties of the Attorney and Solicitor-General would be better or worse performed were these functionaries to be debarred from taking private briefs. The answer to this is, I believe, clear enough ; and an analogous reasoning will apply to the profession of physic.

On the whole matter there are advantages and disadvantages to be balanced against each other on the two sides of this question. In the beginning of this Letter I had the honour to state to your Lordship in the strongest terms such advantages as are supposed to arise from a strict enforcement of duties at naval hospitals, and the evils which may occasionally arise from an abused licence of private practice. The arguments in favour of it seem to be founded on rare exceptions, and apply only to a state of war. In a state of peace the advantages of tolerating private practice seem to be such as greatly to outweigh any detriment or inconvenience that can be alleged or conceived. To say that this liberty never has been or ever can be abused, is to say what cannot be said of any human regulation or institution.

But it is for your Lordship, and not for me to decide, on this matter. I shall be happy if any thing I have stated in this communication can assist your Lordship's excellent judgment, your liberal and candid mind, in coming to a conclusion on this important subject, and have the honour to be, with great respect,

your Lordship's
most faithful and most obedient servant.

P. S. While employed in writing this Letter, I consulted some gentlemen, whom from their age and experience I deemed competent judges of this subject. I found no diffe-

rence of opinion, except that some of them went beyond me in their arguments, maintaining that it would be for the benefit both of the sea and land service, not merely to permit, but to enjoin by positive instruction, all their medical officers to cultivate private practice to the utmost, in so far as it might be compatible with their public duties; that this would be highly conducive not only to keep up a due experience, a proper spirit and interest in their profession, but to fill up their vacant time, and to prevent their losing that habitual readiness which can neither be acquired nor retained but by constant practice and experience, as has already been stated. They further allege, that naval and military practitioners, by mixing with each other, and with civil practitioners in consultation, would mutually enlarge their knowledge, and enable them to treat with more success that variety of cases occurring at hospitals, to which they had not been accustomed in actual war. I am not prepared to go the length of recommending positive injunctions on this subject; for every medical officer of spirit and intelligence will know, without the interposition of authority, how to avail himself of advantages not merely for his own benefit (which is the smallest consideration,) but for that of the service, and of the community at large. I have judged this worthy of mention in order to shew how much general opinion runs in favour of allowing private practice to the medical officers of naval and military hospitals, and with what safety it may be granted under the controul of the good principles of medical officers themselves, and the vigilant inspection of the superintending officer, whose duty will consist in seeing that every one under him is at his post, and in case of default, *from whatever cause*, reprimanding, or in case of great misconduct, reporting to higher authority, with a view to removal or other exemplary punishment.

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